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# VISION THROUGH OPTICAL INSTRUMENTS

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In recent years I think no President has chosen an optical subject on which to address the Society. This is certainly the case since the Physical Society, as we know it now, came into being through the amalgamation of the Physical Society of London and the Optical Society. It is not therefore inappropriate that I should begin by referring to the position in our Society of optics as a part of physics in which many of our Fellows are especially interested.

At first after the amalgamation all optical papers were read at one of the ordinary meetings. When these times proved less convenient, for those more particularly interested, than those of the Optical Society's meetings had been, special meetings were arranged with all-optical programmes. From the first these meetings have been successful, but as they are conditioned by the supply of suitable papers their frequency is uncertain, and it has not been possible to arrange them as long in advance as the other meetings. For various reasons it has seemed desirable to read certain optical papers at the ordinary Friday afternoon meetings; but the special meetings for the reading of most of the optical communications will be continued if this course commends itself to the Society.

These special meetings are perhaps the beginnings of a process which may well extend. The number of papers accepted for publication is such that many have to be read only in title, and among these there may be some which in other circumstances would provoke discussions of particular value to workers in special subjects. I see no reason why groups of Fellows with special interests should not be able from time to time to arrange additional meetings, at times convenient to themselves, for reading papers or holding discussions: nor need it be assumed that London is the place for them. Sooner or later I think we shall have a number of sections holding their own meetings in addition to the general meetings. Moreover, the formation of local branches in suitable centres might well add to the value of the Society to Fellows living at a distance from London; and if this came to pass I hope a number of papers accepted by the Council for publication in the Proceedings would be made available for reading at these centres. The provincial meetings held hitherto have been most enjoyable and successful, and I think the experience of Institutions such as the Electrical Engineers should encourage the Council to allow the Society to expand along similar lines. I ought to add that in saying this I am only expressing my personal views, for no proposal of the kind is or has been before the Council. But I am confident that the Council will always be ready to take whatever action promises to be most effective for the advancement of physics and the diffusion of physical knowledge in this country.

Reverting however to optics, I would remind you that among the special publications of the Physical Society in recent years are two closely connected with the subject I have chosen to speak on to-night—those dealing with the teaching of Geometrical Optics, more particularly in the elementary stage. The second of these, prepared by a Committee on which several experienced teachers served, contains unanimous recommendations on the signs to be given to fundamental magnitudes, and further proposes that the choice of a sign convention for other quantities should be restricted to two systems, which are constructed on different principles. It was recognized that the recommendations would involve schools generally in no little difficulty, for the current text books almost without exception used conventions opposed to some of those recommended. Despite this obstacle, the importance of aligning teaching with technical practice has been widely recognized, and I understand that the recommendations of the report, where they are unanimous, are being more and more extensively followed. There is every reason to expect that before long they will be adopted in all schools and colleges in this country. The publication of the report, and of the papers and discussion which preceded it, has indubitably contributed materially to the advancement of optical knowledge.

It is less easy to speak about the effect of those recommendations where alternatives were proposed. Inevitably a considerable time must elapse before a general preference for one convention rather than another becomes manifest. It will be remembered that one system makes use of a cartesian framework fitted to the optical system. There has been no dearth of technical books using this system, and these have no doubt been useful for constructing elementary courses in which this set of conventions is applied systematically. This system has also the advantage of being familiar in other fields. The alternative proposed by the Committee may be described as the "real-is-positive" system. The absence of school text books using this system has probably proved an initial handicap, but I expect (for I have had no opportunity of investigating it) that this is no longer the case. From personal correspondence I know that this set of conventions appeals strongly to some teachers, and I was recently told by a teaching member of the Committee that indications of a preference for this system among Science masters are not lacking. The efforts they are making to present this, as well as other parts of physics, in ways which appeal to the average boy or girl, will be watched with sympathy and interest by us all. That the tendency is towards the real-is-positive convention is also the conclusion Prof. H. S. Allen (space-systems is his description of the class to which it belongs) reaches in a review of several new optical texts. It is interesting to find that there is a real desire among school and university teachers to achieve the general adoption of a single convention. The weight they attach to uniformity is important because it is in the elementary work that these conventions matter. They are less important in more advanced work, not so much because the more experienced student can be expected to adapt himself readily to any conventions, as because conventions hardly enter into the later stages of the subject.

Technical instruction in ophthalmic optics was not discussed in the report. The foundations of the curvature method now in such general use were laid and built upon by a succession of able teachers armed with an intimate knowledge of conditions in the spectacle industry; hence no changes of convention have been required. The curvature method has the virtue of making pupils realize that the course taken by light is determined by the properties of a finite volume, not those of a vanishingly thin filament, of the media it traverses. For the present-day needs of the spectacle industry this system of optics seems to be well suited; in schools it can with advantage be kept in reserve as an alternative method of developing the theory, and I understand that when time permits it is found valuable in revision courses. But, notwithstanding its undeniable merits, the curvature method appears to me to possess difficulties, and perhaps limitations, of its own, which to some extent arise from the very features which have proved its outstanding virtues. Curiously enough it is in visual problems and in dealing with systems which do not possess rotational symmetry, the very conditions in which it is applied more extensively than any other system, that these difficulties arise.

Using the notation of the two systems that I have referred to—the real-is-positive and the curvature system—I think it will hardly be disputed that the treatment is made to rest on the two equations

$$\frac{1}{u}+\frac{1}{v}=\frac{1}{f} \quad \text{or} \quad L'=L+F$$

and

$$\text{magnification} = -\frac{v}{u} \quad \text{or} \quad \text{magnification} = \frac{L}{L'}.$$

The ideas of focal length or power, and of magnification, are assumed to be adequate for the discussion of all questions that may arise. I have no reason to suppose that in this respect the newer books differ substantially from their predecessors. It is quite possible on these foundations to give a theory adequate for what I call Laboratory Optics. For instance we can deal with the sizes and positions of images, and devise lens systems for photography or for recording with such instruments as thermopiles and photoelectric cells. But when we consider visual instruments all this seems rather inappropriate. We cannot readily measure the overall magnification for a system including the eye, and it is not clear that we are seriously concerned with the power of such a combination. Our interest should surely lie in the properties of the instrument as they affect the appearance of the picture presented to us; in this respect the treatment now followed seems to me unquestionably defective. Indeed we might well wonder whether the writers of modern books have ever looked through a lens or other optical instrument, except to search for pins. Nor is the omission confined to elementary books. I have recently made an extensive examination of many modern works dealing with the more advanced parts of optics.

In only one of these have I found any mention of part of the general theory which I think is of great importance; and even there it is treated so lightly as to suggest that its significance has not been properly appreciated. I wish in this address to point out where I think the current theory needs augmenting, and to indicate some difficulties which those who adopt the curvature system for all purposes should consider. Perhaps it will be as well for me to say at once that in this discussion nothing difficult or abstruse is involved.

Now it is very easy when we are constructing a scientific theory to overlook facts that are familiar to us. The simpler and more obvious the fact the more likely it is to remain below that level of consciousness at which we recognize it as a fact that demands attention. We are going to consider whether we have not formed a habit of overlooking something which we know, but disregard because it is so familiar. On such a quest it may not be unwise to start in the nursery.

I expect most of us still have some recollection of Hans Andersen's tales. The one I would recall now would doubtless be classed by *The Times* under A rather than U, both because it is highly immoral, and because so many morals can be drawn from it. In *The Tinder Box* we read of a soldier meeting a witch as he was on his way home from the wars. This is what the witch tells the soldier:

"When you reach the bottom of the tree, you will find yourself in a large hall: it is quite light, for there are more than a hundred lamps. Then you will see three doors which you can open, for the keys are in the locks. If you go into the first room, you will see, in the middle of the floor, a large box on which a dog sits; it has eyes as big as tea-cups, but you need not mind it. I will give you my blue check apron, which you must spread out upon the floor, then walk straight to the dog, lay hold of it, and set it upon my apron, open the box and take as many pennies as you like. It is all copper money; but if you would rather have silver you must go into the next room. There sits a dog with eyes as large as the wheels of a water-mill, but do not mind that; set it upon my apron and take the money. If however you want gold, you can have that too, and as much of it as you like to carry, by going into the third room. But the dog that sits on the money-box has two eyes, each one as big as the Round Tower of Copenhagen. That is a dog, I can tell you! But never mind him, only put him upon my apron, when he will not hurt you, and take as much gold out of the box as you like!"

Now I suppose we should all share to some extent in the emotion the witch mentions if we suddenly met dogs of this breed. But it is the dogs themselves, or rather the character of their vision, that I want to consider. What would a man appear like to a dog with eyes as big as tea cups? I suppose the soldier expected to be regarded as an insignificant being, if not indeed small and obnoxious enough to be a natural prey. But a reflecting man might have thought, "the curvature of this dog's eyes must be relatively small, and the focal length therefore long; so I shall appear larger to him than I should to an ordinary dog. He will probably be intimidated by my apparent bigness, so I need not fear him." Now whether this argument is correct, or whether it was the blue check apron that kept the dog subdued, the story does not tell us. The witch may have known, but as the soldier cut off her head, her knowledge is not available for us. The soldier himself doubtless cared nothing for these things.

That is one way of introducing the subject. I think I might fairly call it the physicist's way. There is another, the mathematician's. This might run:

"When we are dealing with a pair of conjugate points the quantity

$$\frac{1}{f}-\frac{1}{u}-\frac{1}{v} \quad \text{or} \quad F+L-L'$$

vanishes. What is the meaning of this quantity when the points are not conjugate?"

Of course it requires unusual perspicacity to recognize one question as a translation of the other. To be honest I must admit that they are not exactly equivalent. The equations are not in fact written in their physically most significant form; as they appear at the moment we cannot give a sensible physical meaning to the quantity we are asked to consider.

It will be simpler for us to return to the physicist. Failing the dogs, we make our own eyes bigger by putting a telescope or binoculars in front of them. The kind of thing we then see is no doubt familiar to us all, and noticeable in many newspaper illustrations. A very obvious feature is the way in which distances close up. The newspaper from which my lantern slides have been prepared does not publish pictures in which this effect is very pronounced, but I dare say that many of you can recall seeing extreme examples in some of the evening papers; for instance in a cricket picture the bowler may appear right on the top of the batsman at the other wicket, and the pitch seem only two or three yards long. This effect is familiar to everyone who uses binoculars, and of course an explanation can be offered starting from the usual equations. The line of reasoning generally accepted is, I think, on these lines:

If we have two pairs of conjugate points 1 and 2, from the two equations

$$\frac{1}{u_1}+\frac{1}{v_1}=\frac{1}{f} \quad \text{and} \quad \frac{1}{u_2}+\frac{1}{v_2}=\frac{1}{f}$$

we obtain by subtraction a relation which can be written

$$\frac{v_1 \sim v_2}{u_1 \sim u_2}=\frac{v_1 v_2}{u_1 u_2}.$$

This is expressed in the doctrine that the longitudinal magnification is the square of the lateral magnification. It is simple to show that this is true in any instrument having rotational symmetry. The argument then runs that the instrument forms diminished images of the elements of the scene, and presents these to our eyes as the objects for us to look at. The separations between image planes are reduced very much more than distances in these planes, the one reduction being the square of the other. Thus the compression of distances is fully explained.

Now I suggest that this explanation will not do. The correct comparison is between the unaided eye and the eye plus the telescope. The picture seems "correct" to the eye alone. With the telescope added the magnification is greater than with the eye only, and therefore on this theory distances in the line of sight ought to appear unduly great instead of unduly small when we look through the telescope.

We can consider the matter in another way. The pictures which show the effect are not views through telescopes, but projections on a photographic plate, that is to say on a plane. The illusion, if that is the right word to use, therefore does not depend on distances between image planes at all. In fact the whole of this traditional explanation is of the lucus a non lucendo order. It can be formally shown to be self-contradictory by noting that we can get indistinguishable pictures by enlarging to the same size photographs taken with lenses of different focal lengths.

Clearly we can't leave the nursery yet. Let us see what a learned Oxford don has to tell the children—a U story this time.

"So she set to work, and very soon finished off the cake.

... ... ... ...

'Curioser and curioser!' cried Alice (she was so much surprised, that for the moment she quite forgot how to speak good English); 'now I'm opening out like the largest telescope that ever was! Good-bye, feet!' (for when she looked down at her feet, they seemed to be almost out of sight, they were getting so far off)."

Alice, you will observe, has made the elementary deduction that because her height is increasing her feet appear to be dwindling to nothing. We can express this rather more learnedly by saying that she realized the apparent sizes of well-known objects depend on their distances. But perhaps we are no longer young enough to extract all the wisdom from Lewis Carroll's simple words.

So we must grow up, and abandon the gentle atmosphere of Oxford and the nursery for the austerities of Cambridge.

In his *Compleat System of Opticks*, a book which this year reaches the venerable age of two hundred, Robert Smith has a great deal to say about vision. In this there is nothing surprising, for in his time optical instruments generally were intended for visual use. Forgetfulness of the importance of visual instruments—they are still both economically and socially of far greater importance than all the others—is a quite modern aberration among optical writers. Parts of Smith's text are so well expressed and so apposite for my purpose that I should like to quote him at some length. He says:

"In order to account for several appearances in vision, it is necessary to consider the manner of acquiring our ideas of things by sight. The noted question proposed by Mr Molyneux to Mr Locke, whether a person blind from his birth, being made to see, could by sight alone distinguish a globe from a cube, whose difference he knew by feeling, has been pronounced in the negative by both those philosophers: and this opinion has since been confirmed by the experience of several persons, who receiving their sight from the operation of Couching, could not know any one thing from another, however different in shape and magnitude. Mr Cheselden having given us a very curious account of some observations made by a young gentleman who was couched by him in the thirteenth year of his age, I will here insert it in his own words.

Though we say of this gentleman that he was blind, as we do of all people who have ripe Cataracts, yet they are never so blind from that cause, but that they can discern day from night; and for the most part in a strong light, distinguish black, white, and scarlet, but they cannot perceive the shape of anything: for the light by which these perceptions are made, being let in obliquely through the aqueous humour, or the anterior surface of the crystalline (by which the rays cannot be brought into a focus upon the retina) they can discern in no other manner, than a sound eye can through a glass of broken jelly where a great variety of surfaces so differently refract the light, that the several distinct pencils of rays cannot be collected by the eye into their proper foci; wherefore the shape of an object in such a case, cannot be at all discerned, though the colour may: and thus it was with this young gentleman, who though he knew these colours asunder in a good light; yet when he saw them after he was couched, the faint ideas he had of them before, were not sufficient for him to know them by afterwards; and therefore he did not think them the same, which he had before known by those names. Now scarlet he thought the most beautiful of all colours, and of others the most gay were the most pleasing; whereas the first time he saw black, it gave him great uneasiness, yet after a little time he was reconciled to it: but some months after, seeing by accident a negro woman, he was struck with great horror at the sight.

When he first saw, he was so far from making any judgment about distances that he thought all objects whatever touched his eyes (as he expressed it) as what he felt did his skin; and thought no objects so agreeable as those which were smooth and regular, though he could form no judgment of their shape, or guess what it was in any object that was pleasing to him. He knew not the shape of anything, nor any one thing from another, however different in shape or magnitude.... He was very much surprised that those things which he had liked best, did not appear most agreeable to his eyes, expecting that those persons would appear most beautiful that he loved most, and such things to be most agreeable to his sight that were so to his taste. We thought he soon knew what pictures represented, which were shewed to him, but we found afterwards we were mistaken: for about two months after he was couched he discovered at once, they represented solid bodies; when to that time he considered them only as party-coloured planes, or surfaces diversified with variety of paint; but even then he was no less surprised, expecting the pictures would feel like the things they represented, and was amazed when he found those parts, which by their light and shadow appeared now round and uneven, felt only flat like the rest: and asked which was the lying sense, feeling or seeing?"

The way in which we come to interpret what we see is discussed at some length. One or two later passages will serve as a summary.

"The apparent distance of an object, perceived by sight, is an idea of a real distance usually measured by feeling, as by the motion of the body in walking, or otherwise; and is suggested to the mind by the apparent magnitude of the object in view, if seen alone, (as a bird in the air, or as an object in a telescope or microscope;) but if it be seen with other objects, as it usually happens, its distance is suggested both by its own apparent magnitude and by the apparent magnitudes of other adjoining objects...." (This conclusion is borne out by the slides. It is interesting to notice that even the shadows of objects suffice to modify our impressions.)

"From what has been said it appears to me that the ideas of distance are suggested to the mind by the ideas of magnitudes of objects. Hence it follows that an object seen by refraction or reflection, appears at the same distance from the eye, as it usually does from the naked eye, when it appears of the same magnitude as in the glasses."

Now let us apply this principle to our views of cricket. We see all the figures considerably enlarged, therefore we judge they are fairly near to us. The more distant batsman appears little smaller than the bowler, and so we infer that his distance from us is but little greater. In this way we form the impression that the length of the pitch, roughly the distance between the two men, is much less than the 22 yards required by the laws of the game. This explanation is clearly just as applicable to photographs as to views seen through a telescope. (Incidentally I might remark that Smith discusses the varying appearance of the Sun at different altitudes, a matter recently considered in the correspondence columns of *Nature*. He also deals with the apparent shape of the sky, and on this too reaches a definite conclusion.)

Accepting Smith's explanation as correct, we see that Carroll has given just the least twist to the explanation of Alice's experience. When she looked down at her feet they appeared so small that she knew they must be far away. As for the dogs in the fairy tale, we judge that the soldier would appear to them just as big as a man would appear to dogs of ordinary dimensions, that is agreeably with their previous experience of mankind.

Perhaps someone is thinking, "That sort of thing may be all very well in its place, but it's psychology, not optics. In physics we are concerned only with things we can measure, and they must be measurements on which different observers will agree. Are you going to suggest that everyone will give the same answer if they are asked to say how far away someone appears to be when he is seen through a telescope?" The question is obviously important. Let us hear what we can quote from Robert Smith in reply to this. (See diagrams reproduced in figure 1.)

"An object seen by refraction or reflection, appears at the same distance from the eye, as it usually does from the naked eye, when it appears of the same magnitude as in the glasses. To determine this distance in all cases, I conceive a ray $OA$ to go from the eye at $O$, and after its last reflection or refraction to belong to the focus $\dot{o}$, in the common axis $OCQ$ of all the surfaces; and to meet an object $PQ$ in $P$, placed perpendicular to $OQ$; and that a line $P\varpi$ is drawn parallel to the axis $OQ$ till it meets the ray $OA$, produced, in $\varpi$. Then supposing the object $PQ$ to be removed to the place $\varpi\chi$, and there to be viewed by the naked eye; since it appears under the same angle $\varpi O\chi$ or $AOC$ as it appeared under in the glasses, when it was at $PQ$, it will also appear of the same magnitude and consequently at the same distance from the eye in both cases…."

Smith goes on to name $O\chi$ the apparent distance of the object $PQ$, and $\varpi\chi$ the apparent object. It should be particularly noted, since it is so opposed to ideas prevalent to-day, that the apparent object differs both in size and position from the image presented as the immediate object to our eyes. We may at times have wondered at the ineptitude displayed by students trying to put a pin in the right place to mark the position of an image; if so it has not occurred to us that our surprise could be more appropriately attributed to errors we have cherished in our own minds.

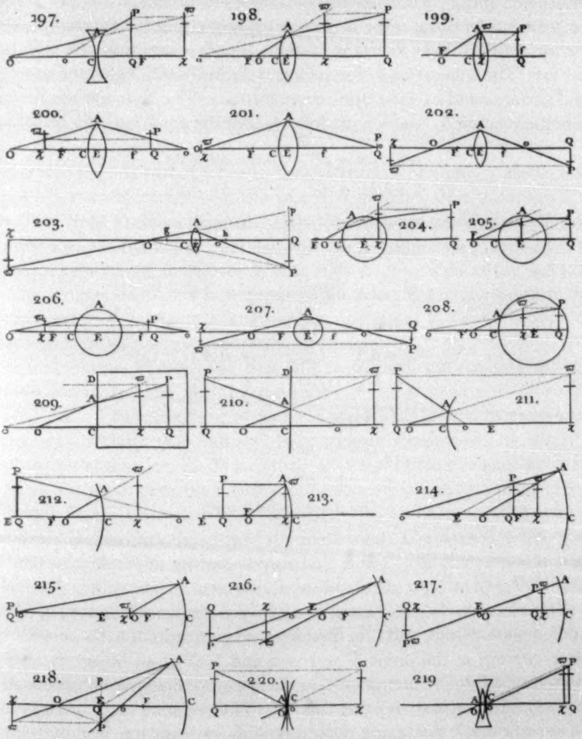

Figure 1.

Though we still retain the name geometrical optics, nowadays we prefer the algebraic to the geometrical form. Let me then recall our procedure briefly. At a refracting or reflecting surface a ray undergoes a change of direction represented by the product of the power of the element and the distance of the point of incidence from the axis. Between successive surfaces the direction remains constant but the distance from the axis changes. On these two facts we are led, in any instrument symmetrical about an axis, to equations of the form

$$-\xi' = ax + b\xi, \quad -\eta' = ay + b\eta,$$
$$x' = cx + d\xi, \quad y' = cy + d\eta,$$

where $x$, $y$ are the displacements from the axis of the point in which the ray meets the first surface, and $x'$, $y'$ the corresponding co-ordinates at the last surface; $\xi$, $\eta$ are the increments in the $x$ and $y$ co-ordinates as we proceed through unit distance along the incident ray away from the system, and $\xi'$, $\eta'$ are similarly defined for the emergent ray. The quantities $a$, $b$, $c$, $d$ are definite functions of the powers of the individual surfaces and of their optical separations. The four are not independent, but satisfy the relation $bc - ad = 1$. It follows that the equations can be solved in the form

$$-\xi = ax' + c\xi', \quad -\eta = ay' + c\eta',$$
$$x = bx' + d\xi', \quad y = by' + d\eta'.$$

Now suppose that the incident ray passes through a point $(X, Y)$ distant $u$ from the first surface, and the emergent ray through $(X', Y')$ at a distance $v$ from the last surface. Then

$$-\xi' = AX + B\xi, \quad -\eta' = AY + B\eta \qquad \text{......(i)},$$
$$X' = CX + D\xi, \quad Y' = CY + D\eta \qquad \text{......(ii)},$$

and

$$-\xi = AX' + C\xi', \quad -\eta = AY' + C\eta' \qquad \text{......(iii)},$$
$$X = BX' + D\xi', \quad Y = BY' + D\eta' \qquad \text{......(iv)},$$

where

$$A = a \qquad \text{......(v)},$$
$$B = b - au \qquad \text{......(vi)},$$
$$C = c - av \qquad \text{......(vii)},$$
$$D = d - cu - bv + auv \qquad \text{......(viii)}.$$

From (v) $A$ is a constant of the system. It will be recognized as the power: for instance if $A = 0$, equations (i) show that corresponding to parallel incident rays we have parallel emergent rays; that is to say the system is telescopic. $B$ is clearly the angular magnification. From equations (ii) $D = 0$ is the condition that $u$, $v$ determine a pair of conjugate points, and $C$ is the linear magnification of the image at $v$. Now if we place our eye at the point $X' = Y' = 0$ and look at an object extending from $(X, Y)$ to the axis, Smith's definition says the apparent distance is $\delta$ where $X = -\delta\xi'$, $Y = -\delta\eta'$. It follows from (iv) that $\delta = -D$. The negative sign arises because our standard direction for measuring distances is away from the lens, whereas Smith takes his positive direction as that in which he is looking, that is towards the instrument. With our conventions the apparent distance of the object is $D$.

At this stage we may note that our Physical and Mathematical statements of the problem have met. Instead of $\frac{1}{u} + \frac{1}{v} - a$, (viii) shows that the form which always has a real meaning is $u + v - auv$, and this meaning, as we have seen, is particularly important in the theory of visual instruments.

In most instruments, when three of the quantities $A$, $B$, $C$, $D$ or $a$, $b$, $c$, $d$ are known, the fourth can be found from the relation $BC - AD = 1$ or $bc - ad = 1$: this is the only procedure left to us if we follow many of our present-day text books, but this method fails to give $D$ when $A$ is zero, that is to say when the instrument is a telescope. The omission to consider this quantity when dealing with telescopes is clearly a serious fault. It is analogous to a disregard of the power or focal length in instruments of other kinds. The only telescope users who are not concerned with $D$ are the astronomers; for the objects they deal with are so remote that to talk of their apparent distance is meaningless.

In a telescope magnifying $m$ times, equation (viii) gives

$$D = d - \frac{u}{m} - vm.$$

As the observer's eye is placed close to the instrument, the only variable quantity is $u$, and the equation indicates that the essential property of a telescope is to make objects appear nearer. (The name suggests this. Just as a microscope is an instrument for looking at objects so that they no longer appear inconveniently small, so a telescope is for looking at objects so that they no longer seem inconveniently far away.) The conversion of real into apparent distances involves a uniform compression inversely proportional to the angular magnification; this ratio is quite different from that which holds for the separation of images.

There is one more result—one that still seems rather surprizing though it has been known for more than two hundred years—that I must not fail to mention. It is due to Cotes. I suppose most of us have sometimes looked through a telescope the wrong way round. Everything then appears diminished or more distant, instead of nearer and so larger. If two men were looking at one another at the same time through the same instrument we should accordingly expect them to appear of very different sizes. Comparison of equations (ii) and (iv), or the symmetry of equation (viii), shows that this expectation is wrong. The first man appears just as far from the second as the second does from the first, and therefore the magnification is the same for both. The key to the paradox lies in the fact that the experiments we usually carry out do not correspond to the conditions assumed in the theory: we do not simply interchange the positions of the observer and the person observed.

Now perhaps someone may feel inclined to ask, why, if an instrument such as a telescope gives the same magnification both ways round, we don't use it in both ways. The answer is in two parts. Firstly utilitarian considerations lead us to prefer one alternative to the other. Let us imagine at a theatre an audience of undergraduates who only know of the other way of using opera glasses. Each of them would be brandishing his glasses at the end of a pole or other contrivance to bring them close to the particular feature of the actress of which he was enamoured, or at least desired to get a much better view; let us, following authority, say her eyebrow. The competition for attaining the desired position of the glasses will mean that most of the audience become disgruntled, and even the actress may not get all the pleasure from it that she should. Hence sensible people take the other alternative, which ensures the greatest happiness for the greatest number.

The second part of the answer is that we do turn the telescope round in this way, but we then give it a different name: we call it a microscope. It was to be expected that the microscope and the telescope should be practically simultaneous inventions. As this use of a reversed telescope is not familiar I have included among the exhibits a telescope used as a microscope.

So far I have confined myself to the particularly simple side of geometrical optics which is regarded as suitable for presentation to the average boy or girl in our schools. My purpose has been to show that if we fail to take into account the idea of apparent distance we are omitting something of real educational value; in fact we are failing to make use of the child's ordinary experience of his surroundings and of what he is most likely to have observed for himself in his private and possibly surreptitious optical experiments; and the theory we present to him is incomplete, unsymmetrical and distorted. I can imagine many teachers loath to abandon

$$\frac{1}{u}+\frac{1}{v}=\frac{1}{f}$$

as the standard form of equation. I think they assume it is more quickly solved than the corresponding form $D=0$. This may be the case if students are supplied with a good table of reciprocals to save arithmetical labour, but I expect this would, to say the least, be abnormal. Failing these tables the alternative form of equation seems to have a slight advantage.

I wish now to discuss briefly the extension of the elementary theory to systems not having symmetry about an axis. Lenses of this class are used very widely in spectacles, so the theory should be regarded as of general interest. I very much doubt though whether many of our opticians or oculists understand much about these systems. As a rule they refrain from thinking of two cylinders unless their axes are in the same plane, and they probably proceed chiefly by rule of thumb, applying the theory of symmetrical instruments in the two principal planes.

Now in fact the theory of unsymmetrical instruments is not particularly difficult —far less difficult than is usually supposed. There is no reason why anyone of ordinary ability should not be able to deal with systems incorporating cylinders with their axes arranged in arbitrary positions. The theory can, in fact, be presented in a form differing remarkably little from that for symmetrical systems. The difference appears in the calculation of the four magnitudes $a$, $b$, $c$, $d$. Instead of being numbers of the familiar type—quantities of the kind the mathematician calls scalars—each is a combination of two vectors. We can deal with any problem we are likely to meet when once we have realized the nature of these vectors and the way they are added and multiplied.*

Before I state what these laws of combination are, it may not be out of place to compare the symmetrical class with the unsymmetrical. I think the former might be described as staid and respectable—almost uninteresting. This cannot be said of the other class. If you have been interested in the Hall of Mirrors at Madame Tussaud's—perhaps one of the best equipped laboratories for teaching the elements of optics in existence—you may agree with me that unsymmetrical instruments are entertaining but neither respectful nor respectable. Some excuse can be offered for them. When they present a man with a view of himself suggestive of a complacent but not contemplative Buddha, with, in place of legs, a rounded base as though he were an outsize in paper weights, they are only doing the kind of thing that was done earlier at Oxford. And we can apply what we have learnt about *D*, for instance the reciprocity law, even to these grotesque pictures.

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* For the theory see Appendix I.

For instance if a man, entertaining his small son with the distorting mirrors, stands, with the boy before him, in front of a mirror of the character I have just described, we may expect that while the father surveys approvingly the modification of his nether regions, the son, unseen, is agitated by the unexpected disappearance of his father's head.

If we will listen, our Oxford philosopher tells us all this plainly enough, and even goes farther: "How are you getting on?" said the Cat, as soon as there was mouth enough for it to speak with.

Alice waited till the eyes appeared, and then nodded. "It's no use speaking to it", she thought, "till its ears have come, or at least one of them."

Misfortunes such as these arise from one part of the body appearing at a very different distance from another part. It is not the kind of thing for the oculist to imitate. We can lay it down that his mission is to enable us to see the different parts of any sufficiently small object at the same apparent distance; and this distance should be the same for all rays entering the eye. We cannot discuss this until we know how to find *a*, *b*, *c*, *d* when the system is not symmetrical. The process involves both the addition and the multiplication of cylindrical powers. When we investigate these processes it immediately appears that the analysis into spherical and cylindrical components is wrong—that is to say it is not the resolution which leads, so far as the optical effects are concerned, to simple intelligible computation or interpretation. It may be the right form of instruction to give to the workman who is to make up the spectacles, but it is certainly not the best form for prescribing them. The relation between the mechanical and scientific systems, as I will call them, is shown in the following table, where the variables S and C signify the quantities regarded as the essential components at the present time.

Table 1. Specifications of spectacle lenses in the mechanical and the scientific systems

| Mechanical system | | | Scientific system | | |
|---|---|---|---|---|---|
| D.Sph. | D.Cyl. | Axis | D.Sph. | D.Ast. | Axis |
| $+S$ | $+C$ | $n^\circ$ | $+S+\tfrac{1}{2}C$ | $\tfrac{1}{2}C$ | $n^\circ$ |
| $+S$ | $-C$ | $n^\circ$ | $+S-\tfrac{1}{2}C$ | $\tfrac{1}{2}C$ | $(90+n)^\circ$ |
| $-S$ | $+C$ | $n^\circ$ | $-S+\tfrac{1}{2}C$ | $\tfrac{1}{2}C$ | $n^\circ$ |
| $-S$ | $-C$ | $n^\circ$ | $-S-\tfrac{1}{2}C$ | $\tfrac{1}{2}C$ | $(90+n)^\circ$ |

The correct division is into a spherical component of power equal to the mean of the principal powers, and therefore also to the mean of the powers of any two perpendicular sections, and an astigmatic component. The latter will perhaps be most readily understood if I describe it as a combination of two equal cylinders, of opposite signs, with their axes at right angles. The power of the astigmatic lens is numerically equal to that of either cylinder. If its axis is taken to be that of the positive component its sign will always be positive.

It should be observed that with this system every specification is unique. There is only one way of writing it. Consequently the subject of the transposition of prescriptions disappears. In what follows I shall use the scientific system, denoting the magnitude of the spherical component by $S$ and that of the astigmatic component by $X$.

That this is the correct method of analysis of a mixed lens seemed so obvious that I had a shock when a passage in a book by one of my optical friends implied, as I thought, that Stokes held a different opinion. But reference to Stokes' paper showed that my fear had been needless; the term astigmatic lens, which I have used to designate the non-spherical component, is the expression employed by Stokes, and was presumably coined by him.

The vector construction for the addition of astigmatic lenses which is given in ophthalmic text books, is originally due to Stokes, though sometimes attributed to others. The essential feature is that the angle of the optical vector is double the angle of the lens axis. (See Appendix I.) Exactly the same law holds for the spherical components also.

If the last statement seems surprising it is only because the vector nature of spherical power has not been realized. An alteration in its angle implies a rotation of the image field relatively to the object field. Rotation of this kind is easily taken into account when we build up our theory on the changes suffered by a typical ray; but less easily and naturally, I should expect, on the curvature system. It should be interesting to know how those who use this system exclusively would discuss this property with their pupils.

How a spherical power comes to have an angle associated with it appears from the law for the multiplication of powers, which may be stated as follows:

To ascertain the nature of any product only the astigmatic factors are considered. The product of an even number of astigmatic factors is of spherical type, that of an odd number is astigmatic. The angle associated with any product is a function of the angles of the elementary astigmatic components. It is equal to the angle of the last component minus the angle of the preceding combination. Thus if $\phi_r$ is the angle of the astigmatic power $X_r$, the angle $\theta$ of a spherical contribution containing astigmatic factors $X_1$, $X_2$, $X_3$ ... $X_{2n}$ is given by

$$\theta = -\phi_1 + \phi_2 - \phi_3 + \ldots + \phi_{2n},$$

and the angle $\phi$ of an astigmatic contribution containing the factors $X_1$, $X_2$, $X_3$ ... $X_{2n+1}$ by

$$\phi = \phi_1 - \phi_2 + \phi_3 - \ldots + \phi_{2n+1}.$$

Let us apply the angle law in some simple cases. If we look through an instrument at a line (which counts as an astigmatic element) inclined at an angle $\alpha$ with the axis, the inclination $\beta$ of its image will be determined by prefixing the angle $\alpha$ to $\theta$ or $\phi$ as the case may be. Thus in an instrument of spherical type

$$\beta=\alpha+\theta,$$

and in one of astigmatic type

$$\beta=\phi-\alpha.$$

Now suppose the instrument is kept stationary and the object rotated—say $\alpha$ is continuously increased; the object and image will revolve in the same sense if the instrument is of the spherical type, and in opposite senses if it is of the astigmatic kind, corresponding to increasing and decreasing values of $\beta$ respectively.

In instruments having components of both kinds, superposition of these opposing effects tends to cause a confused image; unless the rays are selected in some way, as for instance by a small stop, only lines running in two fixed directions at right angles to one another are sharply defined.

Again if the object is fixed and the instrument is rotated, the angle $\phi$ of each astigmatic lens is increased by twice the angle of rotation, say $\chi$. In $\theta$ the number of $\phi$'s is even, and half occur with positive and half with negative signs. It follows that $\theta$ is unaltered by rotation of the entire instrument, and hence $\beta$ is also unchanged. In $\phi$ on the other hand the positive terms number one more than the negative terms, and $\phi$ therefore increases by $2\chi$. Since $\beta$ increases by the same amount as $\phi$, the image in the astigmatic case revolves twice as rapidly as the instrument and in the same sense.

As a rule in instruments containing astigmatic lenses both effects are present simultaneously, and what we see depends on the relative magnitude of the two. When an optician examines a lens he finds whether a cylindrical correction has been incorporated by looking through the lens at perpendicular cross lines while he rotates it about its axis. The apparent position of the object of course depends on the function $D$.* If the apparent direction of the cross lines does not change the lens surfaces are spherical; if a cylindrical component is present he expects to find what he calls the scissors movement—the lines have a to and fro angular motion like the blades of a pair of scissors in use. But this is only part of the story. By making a suitable choice of object and lens distances we can alter the relative magnitude of the spherical and astigmatic components of $D$ almost as we like, and instead of the scissors movement we can cause the lines as seen through the lens to perform complete revolutions. We can obtain both effects simultaneously by using two different distances, e.g. cross lines near to the lens and any remote object. The cross lines will then show the scissors effect while the trees and buildings in the distance turn somersaults. Figure 2 shows the changes of apparent direction of perpendicular cross lines with a simple cylindrical lens. The change from oscillation to continuous rotation is particularly interesting.

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* In the general case apparent position seems more appropriate than apparent distance, since apparent rotation of the object is included in $D$.

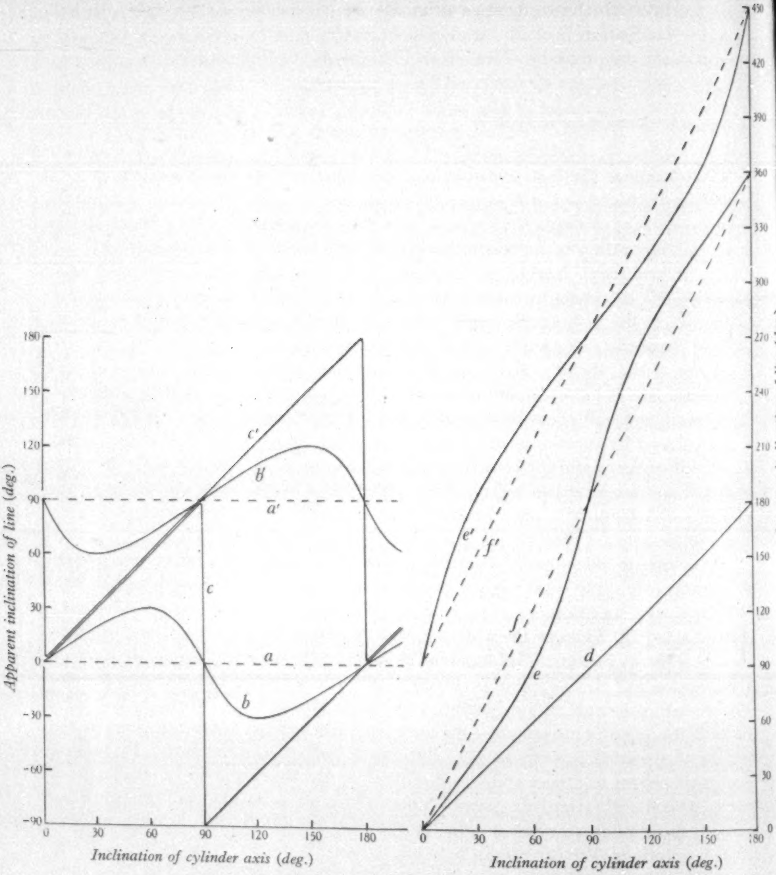

Figure 2. *The scissors effect and its variations.* The lens is a cylinder of power 2, i.e. $S=1$, $X=1$. The distance of the observer's eye from the lens is $\frac{2}{3}$. $a$, $a'$, cross lines in contact with lens: spherical effect. $b$, $b'$, cross lines distant $\frac{2}{3}$ from lens: scissors effect. $c$, $c'$, cross lines just less than 2 from lens: limiting form of scissors effect: both lines parallel to lens axis. $d$, cross lines just more than 2 from lens: both lines parallel to lens axis: continuous rotation begins. $e$, $e'$, cross lines at infinity: oscillation superposed on continuous rotation. $f$, $f'$, the pure astigmatic effect: cross lines appear to rotate twice as fast as the lens: only attained with a virtual object under the conditions assumed here.

As far as I know the first instrument made to exhibit the rotational property was constructed by Burch of Reading University College. It consisted of two (equal) cylindrical lenses with their axes parallel, separated by a distance equal to the sum of their focal lengths. I have an illustrative model. It does not produce sharp images because Burch failed to take apparent distance into account. Consequently there is a large amount of astigmatism: the images of lines in the two principal planes of the telescope are twice the length of the instrument apart. The same defect appears in other instruments described in the same paper. Burch must have possessed remarkable powers of accommodation, not to mention tolerance of astigmatism, to be able to use these instruments in some of the ways he describes.

A true astigmatic instrument—we might perhaps say a panastigmatic instrument—must have all the four quantities $A$, $B$, $C$, $D$ of the pure astigmatic type. This involves at least three lenses, each as a rule with both spherical and astigmatic components. A simple unit power telescope of this kind can be made with three similar lenses with their axes parallel, and each separation equal to the focal length of the astigmatic component and to twice that of the spherical component. Real images can be obtained. For instance the object and image planes can each be one quarter the length of the whole instrument from the extreme lenses. As all the axes lie in one plane it is easy to verify that this construction gives the properties mentioned.

Another interesting system is of the spherical type, but the images are neither inverted nor erect. This effect can only be obtained by adjusting the axes of the astigmatic components to lie in different planes. At least four component lenses are necessary. A unit telescope can be made from four similar lenses spaced at equal intervals. To secure an image rotated through an angle $\epsilon$ relative to the object the axis of each lens should make an angle $\frac{1}{4}(\pi-\epsilon)$ with that of the preceding lens. Taking the separation between the lenses as the unit of length, the power of the spherical component should be 2 and that of the astigmatic component $(2\sin\frac{1}{2}\epsilon)^{\frac{1}{2}}$. The range of possible constructions is shown in figure 3. For instance to turn the image through a right angle the axes of successive lenses make angles of $22\frac{1}{2}°$, and the powers are 2 spherical and $2^{\frac{1}{4}}$ astigmatic. In the notation of the optician the specification would be $2-2^{\frac{1}{4}}$ spherical, $2\times 2^{\frac{1}{4}}$ cylindrical, a ratio of about 15 : 44 or approximately 1 : 3. If we put two telescopes of this kind in series we rotate the image through two right angles, that is to say we apply the factor $-1$. The single telescope therefore is a representation of $\sqrt{-1}$. I should get into trouble with my mathematical friends if I called it $i$, so I will go as far as I dare and suggest the name $j$.

Though it would be a mistake to assume that systems of the kind just considered would serve no useful purpose, we have to realize that the study of astigmatism is chiefly pursued with the intention of eliminating it, or at least of compensating for its presence. In the most important case which calls for consideration there are only two astigmatic components. From what has been said already it is obvious that the axes of these components will be either parallel or perpendicular.

Suppose the two components, say $X$ and $X'$, are separated by a symmetrical system represented by the functions $a, b, c, d$. When the astigmatic components are included these are replaced by $a^*, b^*, c^*, d^*$, where

$$\begin{aligned} a^* &= a + Xb + cX' + XdX', \\ b^* &= b + dX', \\ c^* &= c + Xd, \\ d^* &= d. \end{aligned}$$

From these equations it is obvious that the astigmatic lenses can only neutralize one another if $d = 0$, that is to say if the one lens is situated at a real image of the other. This result is important.

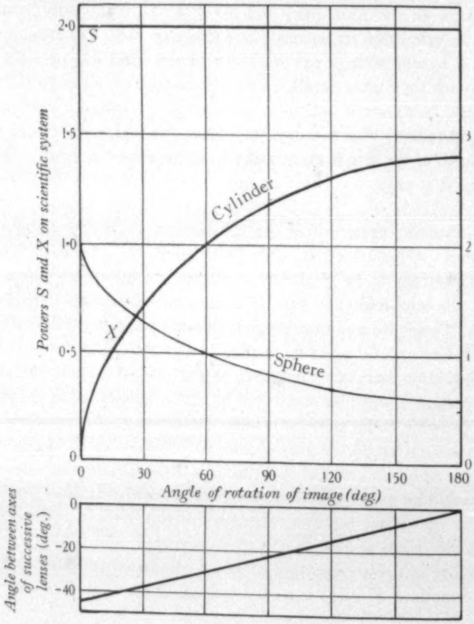

Figure 3.

Let us first consider it from the point of view of the instrument maker. We learn from it that, as a rule, a fault which is remediable by figuring a surface can only be perfectly corrected by working one particular surface, which is the one where the error arises. It has to be remembered that the formation within an instrument of real images of its components is exceptional. It is true that correction can be secured by working several surfaces no one of which is imaged on another, but as the method of doing this involves preliminary theoretical investigation it can hardly be regarded as a suitable alternative for the workshop.

In addition to the vanishing of $d$ the relation $Xb+cX'=0$ must be satisfied. Since in this case we have $bc=1$ the correcting power $X'$ is given by $X'=-Xb^2$. The negative sign indicates that the axes of the two astigmatic lenses must be perpendicular to one another. It will be recalled that $b$ represents the angular magnification. This astigmatic theory is of course relevant to the design of such instruments as photographic lenses, where astigmatic differences of power arise owing to the obliquity of the incidence of light on the surfaces. One of the exhibits shows the astigmatic effect of a tilted spherical lens. The object as seen by light passing outside this lens remains stationary as the instrument is rotated; but as seen through the lens it exhibits the rotation characteristic of an astigmatic lens.

From the point of view of the oculist the condition $d=0$ implies that astigmatism due to want of sphericity in the cornea can only be fully corrected by means of contact glasses or alternatively by appliances far more clumsy than spectacles. The oculist has also to remember that the eye rotates to some extent in its socket in the course of its normal movements. When investigating what form of spectacle lens will give the most satisfactory vision, the inclination of the axes of the astigmatic components of the lens and of the eye on its extreme excursions should be taken into account. The general theory of astigmatic systems is therefore required for a full understanding of common spectacle problems. It is also necessary for others with which I have not attempted to deal—for instance the way in which judgment of distances on the principle considered here is modified for near objects by binocular vision, and the effect of glasses for players in games such as cricket and lawn tennis, where the path of a ball in rapid motion has to be recognized. These fall outside the limits of the elementary theory to which I am restricting myself to-night.

I can however give some examples of the application of the theory to simple instruments. I take reflecting telescopes of unit power as illustrations. Spherical mirrors can be employed, since astigmatic powers are introduced by letting the light fall obliquely on the surfaces. I select three types, which may be named the *chimney*, the *cube*, and the *steps* respectively. As with all other telescopes it is insufficient for clear vision through them to ensure that parallel incident rays emerge in parallel directions and that the magnification shall not vary for rays in different planes. In addition to these conditions it is essential to control the apparent position. In all three examples this has been done by making the apparent distance $D$ zero for an object distant $t$ in front of the first mirror and a real image distant $t$ from the last mirror. Perhaps I might remark here that it is often not realized that a telescope can form real images of objects within a limited distance of the instrument. For example it is possible to use a telescope to project pictures on a screen.

The *chimney* telescope may be represented symbolically by the expression

$$\left[t\begin{pmatrix}p\\s\end{pmatrix}t\right]^n,$$

which implies a succession of $n$ systems, each of the type indicated by the letters within the square brackets. The $t$ denotes a distance measured along a principal ray, so that $2t$ is the distance between the vertices of successive mirrors. The $p$ and $s$ denote the primary and secondary planes of reflection; the events which occur to rays in one plane are shown in the upper position, and those experienced by rays in a perpendicular plane in the lower position. In this expression all the $p$'s occur at one level, and all the $s$'s at another level. This implies that all the mirror vertices and centres of curvature lie in a single plane. The regular formations have the vertices either on two parallel straight lines or at the corners of a regular polygon. The chimney describes the former. The path of the ray reflected at the vertices of the mirrors is shown in figure 4.

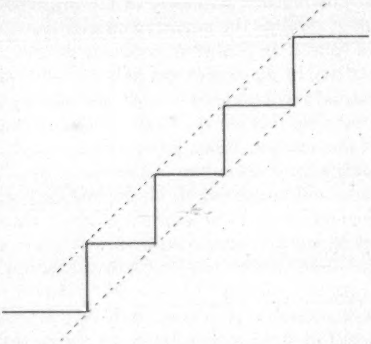

Figure 4. Path of central ray in chimney reflecting telescope.

The system is severely limited in its degrees of freedom. If $r$ is the radius of each mirror and $\pi-2\delta$ the deviation suffered by the central ray at each reflection, we must have

$$\cos\delta=\frac{\sin\dfrac{h\pi}{2n}}{\sin\dfrac{k\pi}{2n}},$$

$$\frac{t}{r}=\sin\frac{h\pi}{2n}\sin\frac{k\pi}{2n},$$

where $h$ and $k$ are integers. If $h$ and $k$ are both even, the telescope is erecting: if both odd, inverting: and if one is odd and one even it is panastigmatic.

The next construction is represented by

$$\left[t\begin{pmatrix}p\\s\end{pmatrix}2t\begin{pmatrix}s\\p\end{pmatrix}t\right]^{n}.$$

Rays in the primary plane for one reflection lie in the secondary plane at the next. Consequently we cannot have more than three consecutive mirror vertices in the same plane, and the plane containing those of mirrors 2, 3, 4 is perpendicular to the plane in which those of mirrors 1, 2, 3 lie. If the deviation is a right angle one regular arrangement leads to the central ray tracing out a number of the edges of a cube, as shown in figure 5. The conditions for clear vision are less restricted than in the previous telescope. They only amount to the one relation

$$\frac{4t}{r} = \cos\delta + \sec\delta \pm \left\{\cos^2\delta + \sec^2\delta + 2\cos\frac{h\pi}{n}\right\}^{\frac{1}{2}},$$

where $h$ is not zero or a multiple of $n$. The telescope is inverting or erecting according as $h$ is odd or even. The astigmatic variety is ruled out.

The third type is represented by

$$\left[t\begin{pmatrix}p\\s\end{pmatrix}2t'\begin{pmatrix}s\\p\end{pmatrix}2t\begin{pmatrix}s\\p\end{pmatrix}2t'\begin{pmatrix}p\\s\end{pmatrix}t\right]^n,$$

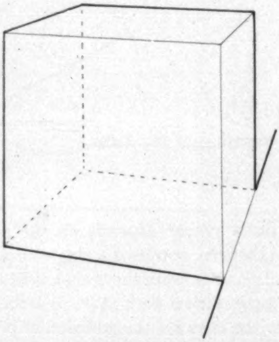

Figure 5. Path of central ray in cube reflecting telescope.

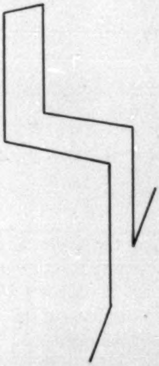

Figure 6. Path of central ray in steps reflecting telescope.

and it is easy to verify that if we divide the system after any even number of reflections the next four mirrors will have their vertices in a single plane, and that at the next division the new plane will be perpendicular to its predecessor. One arrangement of the path resembles a short flight of stairs in outline, hence the name proposed. In addition to other solutions we get an erecting telescope for all values of $n$ if we make

$$r(\cos\delta + \sec\delta) = 2t(\cos^2\delta + \sec^2\delta) = 4t'.$$

Although all these telescopes satisfy the conditions for clear vision by rays near the central ray, they will be found to behave very differently with large fields of view. The chief factor which makes for good vision over a large field is symmetry. It is not an unprofitable study to consider how one system differs from another in this respect. Among the systems I have described the best definition has been obtained with the steps; it may be arranged (see figure 6) as steps up one side of a wall and down the other, a mirror on one side being exactly opposite a mirror on the other side. The worst results have been found with the chimney arrangement.

If some of the systems I have described appear to you to be rather fantastic creations, I would say that situations have arisen where they can be usefully applied, and I have no doubt that other uses for such instruments will be found.

In conclusion I must express my thanks for the assistance I have received from many helpers. To Mr Philpot, director of the British Scientific Instrument Research Association, I am deeply indebted for the aluminizing of the mirrors used in the reflecting telescopes. Colleagues at the National Physical Laboratory have helped in numerous ways: it is only fitting that I should mention in particular Dr Anderson, Mr Buxton, Mr Pinfold and Mr Turl, without whose assistance I should not have been able to bring these model systems for you to see. For several of the pictures I have shown on the screen I am indebted to the two youngest members of my family.

## APPENDIX I

If in the second order surface equation

$$2z = ax^2 + 2bxy + cy^2,$$

we write $x = r \cos \psi$, $y = r \sin \psi$, we obtain an equation of the form

$$\frac{2z}{r^2} = \frac{a+c}{2} + d \cos 2 (\psi - \chi),$$

where $\chi$ is the angle of a principal section. Since power depends on the rate at which the path length varies with $r^2$, it follows that the power of a single surface is of the form $S + X \cos 2 (\psi - \chi)$. The optical angle is thus twice the mechanical angle.

If two surfaces of this type occur in contact, the optical path at $(r, \psi)$ is the sum of those of the elements. It at once follows that the laws for the addition of powers are

$$S = S_1 + S_2,$$

$$X \{\phi\} = X_1 \{\phi_1\} + X_2 \{\phi_2\}, \quad \phi = 2\chi,$$

where the second equation implies that the addition is vectorial. This construction, first given by Stokes, is illustrated in figure 7.

Since the normal at $(x, y)$ has direction cosines in the ratios $ax + by$, $bx + cy$, $-1$, the directions of a ray refracted at this point satisfy

$$\frac{\xi' + \xi}{ax + by} = \frac{\eta' + \eta}{bx + cy} = -(\zeta' + \zeta) \simeq \mu - \mu';$$

these equations are a simplification of the more general type

$$\begin{aligned} -\xi' = {} & \xi (b \cos \beta + b' \cos \beta') + \eta (-b \sin \beta + b' \sin \beta') \\ & + x (a \cos \alpha + a' \cos \alpha') + y (-a \sin \alpha + a' \sin \alpha'), \\ -\eta' = {} & \xi (b \sin \beta + b' \sin \beta') + \eta (b \cos \beta - b' \cos \beta') \\ & + x (a \sin \alpha + a' \sin \alpha') + y (a \cos \alpha - a' \cos \alpha'). \end{aligned}$$

Assume that this form holds for each of two parts of a complete system. The equations connecting the variables in the two parts are

$$\xi_1' + \xi_2 = \eta_1' + \eta_2 = 0, \quad \frac{x_2 - x_1}{\xi_1'} = \frac{y_2 - y_1}{\eta_1'} = \frac{t}{\mu},$$

where $t$ is the length of the path between the components in the intervening medium of index $\mu$.

In the expression for $\xi_2'$, $\eta_2'$ in terms of $\xi_1$, $\eta_1$, $x_1$, $y_1$, derived by eliminating the remaining variables, the coefficients of $x_1$ and $y_1$ clearly contain terms having $\frac{t}{\mu}$ and an $a$ from each component as factors. Considering these terms alone it is easy to verify that they make contributions to $\xi_2'$ and $\eta_2'$ of the same type as has been assumed for each part. If we denote these types by the notations $a\,(\alpha)$ and $a'\,\{\alpha'\}$ respectively, the terms we are considering are $\frac{t}{\mu}$ times

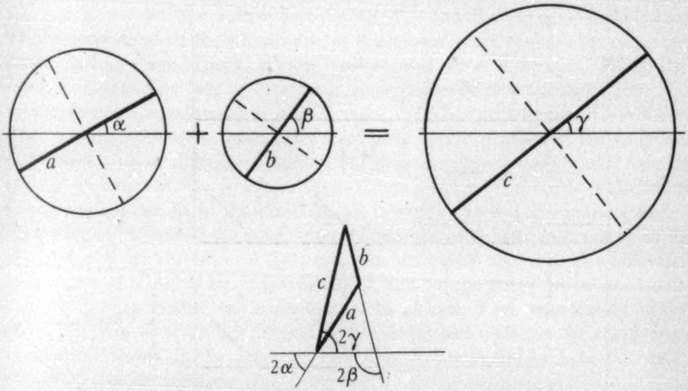

Figure 7. Stokes' construction for the addition of astigmatic powers.

$$a_1 a_2\,(\alpha_2 + \alpha_1) + a_1' a_2\,\{\alpha_2 + \alpha_1'\} + a_1 a_2'\,\{\alpha_2' - \alpha_1\} + a_1' a_2'\,(\alpha_2' - \alpha_1').$$

This shows that the products $a_1 a_2$ and $a_1' a_2'$ are of the kind characteristic of the spherical group, and $a_1' a_2$ and $a_1 a_2'$ of the other, the astigmatic group.

When we are dealing with individual surfaces, the angle for each spherical component is zero. It readily follows from the equations we have found for combining angles that in all groups the resultant angle is obtained by summing the angles of the astigmatic factors with their signs alternately positive and negative when arranged in their proper order, the sign of the final term being always positive.

## APPENDIX II

As the representation of the properties of optical systems of the most general kind by vectors of two distinct groups enables graphical methods to be employed, and as the procedure is new, it may be helpful to portray the stages by which the properties of the telescope mentioned on p. 879 are reached. This has been done in figure 8, each stage being represented by a separate scene. Instead however of taking the displacement from one lens to the next as an elementary event, it has been replaced by the joint effect of

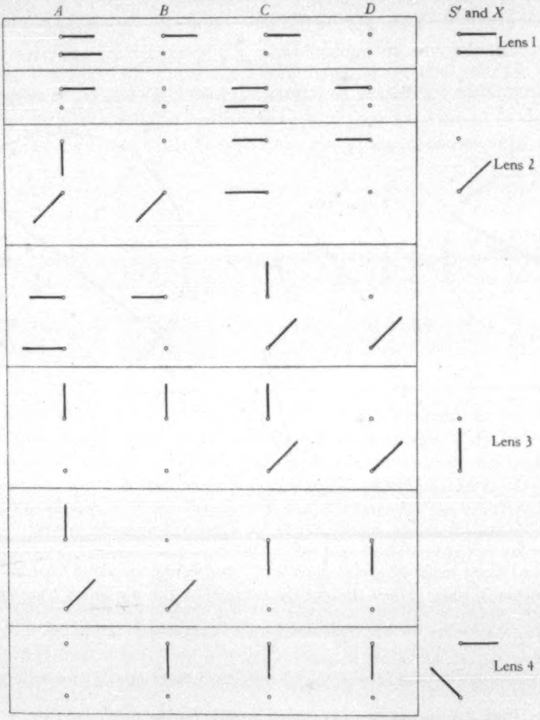

Figure 8. Graphical representation of the action of telescope '$j$'.

(i) half the spherical power at the beginning of the interval;
(ii) the displacement;
(iii) half the spherical power at the end of the interval.

The joint effect of the three is to replace $A, B, C, D$ by $C, D, -A, -B$ respectively. All that is left is to combine the effects of the remaining half of the spherical powers of the external lenses, and the astigmatic effects of all. The insertion of these powers does not alter $C$ and $D$, but the products of the power to be included and $C$ and $D$ respectively are to be added to $A$ and $B$. The directions ascribed to the vector products are to be determined by the rules stated on p. 876.

In figure 8 the spherical components of each of the four quantities $A$, $B$, $C$, $D$ are given immediately above the astigmatic components. To begin with we have the representation of free space—unit vectors for $B$ and $C$ in the upper line and all else zero. The powers to be incorporated are represented on the extreme right. Multiplying $C$ and $D$ by these has the effect, shown in the first scene, of adding these powers to $A$. This gives the initial $A$ and $B$. In accordance with the explanation just given, the four actors first posing as $A$ and $B$, having made their bow, subsequently stand on their heads and in this posture walk through the next three scenes, making a half-right turn on going from one scene to the next. Before they finally disappear they help to determine the character of their successors.

For instance a particular $A$ and $B$ make their last appearance in scene two. Their successors in scene three differ from them by the product of the astigmatic power shown on the right and $C$ and $D$. The non-zero elements of $C$ and $D$ have the angle $\pi$, and that of the power to be introduced is $\frac{1}{4}\pi$. The vector additions to scene two therefore lie in the direction $\frac{1}{4}\pi - \pi$, or $\frac{5}{4}\pi$. In magnitude the spherical components of $C$ and $D$ are equal to unity, and the astigmatic components of the new $A$ and $B$ are therefore $2^{\frac{1}{2}}\,\{\frac{5}{4}\pi\}$, since the old values are zero. The astigmatic components of $C$ and $D$ are of magnitude $2^{\frac{1}{2}}$ and 0 respectively. After multiplication by the new power the contributions to $A$ and $B$ are respectively $2^{\frac{1}{2}}\,(\frac{5}{4}\pi)$ and 0. Adding these to the previous values 1 (0) and 0 we get the components 1 $(\frac{3}{2}\pi)$ and 0 shown in the diagram. The other stages should be followed readily enough by similar treatment.

It will be realized that in practice the work would be abbreviated by cutting out the repetitions which occur. The figure contains almost four times as many representations as are necessary.

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# THE FOCUSING PROPERTIES OF THE ELECTROSTATIC FIELD BETWEEN TWO CYLINDERS

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*ABSTRACT.* A general survey of the focusing properties of an electron mirror formed by two cylinders has been made. Photographs of the images formed are shown, and the relation between object and image distances for different voltage ratios on the cylinders are obtained. A special case of multiple reflections is described. Some qualitative results relating to the behaviour of the two-cylinder lens at very high voltage ratios are given, and the formation of intermediate images in both lenses and mirrors is discussed. Finally, a qualitative description of the focusing properties of two cylinders for voltage ratios from $-\infty$ to $+\infty$ is given.

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## §1. INTRODUCTION

SEVERAL papers$^{(1,2,3,4,5,6)}$ dealing with the electron mirror have already appeared. These have in general, however, been concerned with the Einzel lens and its corresponding mirror. A more recent paper$^{(7)}$, which was published during the preparation of the present one, has, however, included the case of a cylinder and a plane or curved electrode. The Einzel lens is one of the most complicated types, since under certain conditions the field near the axis behaves as a lens, while the field near the electrodes behaves as a mirror. Moreover, when the system finally becomes entirely a mirror the range of focal lengths is still rather limited. In the following work a very simple case, the mirror formed between two cylinders, has been chosen for investigation. This arrangement lends itself readily to experiment, and the results, together with some recent results relating to the lens formed between two cylinders, fill in the gaps in our knowledge of the focusing properties of two cylinders maintained at different potentials. The focusing properties of the lens formed between two cylinders have been studied over a limited range by Epstein$^{(8)}$, and a later paper by Klemperer and Wright$^{(9)}$ includes the case of decelerating lenses. No measurements have yet been published for the case in which the final cylinder is maintained at a negative potential with respect to the cathode, or for that of very high potential ratios on the two cylinders. The first of these constitutes the electron mirror and the second is concerned with the formation of intermediate images in lenses. The electron mirror will be dealt with first.

## §2. THE TWO-CYLINDER ELECTRON MIRROR

Consider two cylinders, figure 1 A, with an electron beam entering from the left. The cylinder on the right is maintained very much negative to the cathode, while the one on the left is maintained positive. Under these conditions a zero equipotential surface is formed in the region within the two cylinders, and its exact location can be determined by means of a field plot in an electrolytic trough. The beam of electrons travelling to the right in this system is first decelerated and then returned towards the cathode, the direction of each ray being determined by the angle of incidence and the shape of the equipotential surfaces. This return is brought about by total reflection of the electrons in the neighbourhood of the zero equipotential surface and the axially symmetric field focuses the electrons in the same manner as the field of an electron lens. The position of the zero equipotential surface can best be described as follows. It is conventional in field plots of two-electrode systems to consider one electrode at zero and to label the equipotential surfaces in terms of percentage of the total potential-difference. We can therefore in the case of the electron mirror refer the results for the two cylinders at potentials $V_1$ and $V_2$ to the case where one is at potential $(V_1 - V_2)$ and the other is at zero. The field between two equal cylinders is well known, and if we describe the zero equipotential surface in terms of it we immediately know the shape and position of

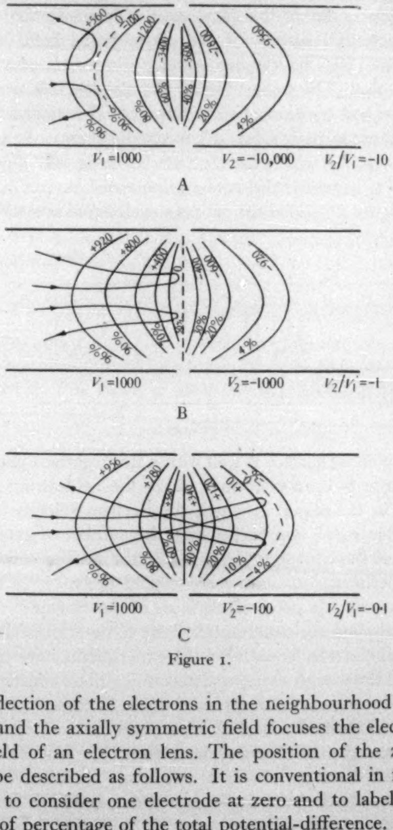


Figure 1.

the surface. It is thus clear that when the cylinders are at potentials $V_1$ (positive to the cathode) and $-V_2$ (negative to the cathode) then the total potential-difference is $(V_1 - V_2)$ and any equipotential surface $V_x$ relative to the cathode will be $(V_x - V_2)$ relative to the second electrode and hence the equipotential surface is equivalent to the 100 $[(V_x - V_2)/(V_1 - V_2)]$ per cent equipotential surface of the conventional field plot. Thus, for example, in figure 1 A where $V_1 = 1000$ and $V_2 = -10{,}000$ the equipotential surface $+560$ is the same as the $(560 + 10{,}000) \div 11{,}000 \times 100$, or 96 per cent equipotential surface of the conventional field plot, and the zero equipotential surface will be $(10{,}000 \div 11{,}000) \times 100$ or 90·8 per cent in the conventional field plot. The behaviour of the electron beam can in general be judged by the shape and position of the equipotential surfaces. Figure 1 A shows the approximate electron path when $V_2$ is strongly negative, giving a diverging field in the region through which the electron beam passes. Figure 1 B shows the path for $V_2/V_1 = -1$, in which the zero equipotential surface is midway between the cylinders, and figure 1 C shows the path of the electrons when $V_2/V_1$ equals $-1/10$ and the zero equipotential surface is well within the negative cylinder. In this latter case the beam is seen to cross over once inside the field owing to the very strong converging action in the region in which the electron-velocity is low. As will be explained later, when $V_2/V_1$ approaches zero the number of cross-overs inside the field increases, since the converging portion of the mirror becomes stronger and stronger. The experiments to follow are considerably easier to interpret when reference is made to the equipotential surfaces of the system.

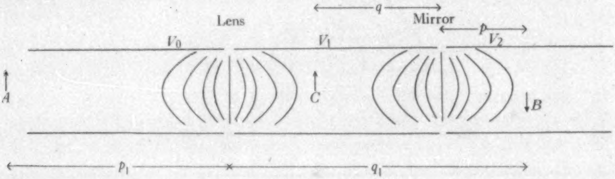


Figure 2.

Epstein$^{(8)}$ has applied the usual optical laws to determine the principal planes and focal lengths of electron lenses. Similar experiments have been carried out in this laboratory, and it was with a view to determining these constants for the electron mirror that the following experiments were made. For such a determination it is necessary to know at least two magnifications and the corresponding distances of object and image for a given focusing system. Hence, in order to cover a large range of voltage ratios, the distances of object and image must be variable over a large range. Figure 2 shows diagrammatically the system used. The lens formed between the first two cylinders at potentials $V_0$ and $V_1$ produces a real inverted image of $A$ at $B$ when the third cylinder is also at potential $V_1$. When the third cylinder is maintained at potential $V_2$ (negative with respect to the cathode) the image $B$ becomes the virtual object for the mirror formed between cylinders 2 and 3, which produces the final image at $C$ by reflection. Now for a given value of $V_2/V_1$, i.e. a given reflecting system, two values of $p$ (the distance of the object), $q$ (the distance of the image) and $m$ (the magnification) must be obtained. In order to cover the whole range, curves of $p$ and $m$ against $V_2/V_1$ are obtained for at least two values of $q$. The distance $p_1$ is fixed, and $q_1$ can be varied by altering $V_1/V_0$. Also $q_1$ can be obtained from the known relations for two equal cylinders, which have previously been obtained. From a knowledge of $q_1$ the quantity $p$ can be obtained, while the varying position of the image $C$ is observed on a movable screen, thus giving $q$. The mirror magnification $m$, which is the size of the image $C$ divided by the size of the image $B$ is obtained from the observed magnification of $C/A$ and the previously known magnification $B/A$. It must be noted here that limitations in the variation of $q$ (three tube-diameters) for which $p$ could be measured with reasonable accuracy kept the two curves of $p$ against $V_2/V_1$ so close together that no determination of the focal length and principal planes could be made. The curves of $p$ and $m$ against $V_2/V_1$ for various values of $q$ are nevertheless given, and are of considerable use in setting up mirror systems. The above method would be satisfactory for an investigation of the mirror over a limited range, but the experiments here described were essentially a preliminary survey of the mirror over the entire range.

## §3. EXPERIMENTAL ARRANGEMENT

The experimental arrangement for investigating the electron mirror is shown in figure 3. The electrons are supplied by a hot oxide cathode $C$ surrounded by a shield $A$ at cathode potential. The beam of electrons is accelerated by the potential $V_0$ on the adjacent cylinder and passes through stops $S_1$, $S_2$ and $S_3$, and also through a fine-mesh nickel gauze $G$. An electrostatic lens is produced, between this cylinder and the following one at potential $V_1$, which serves the purpose of varying the position of the mirror object. The cylinder at potential $V_1$ also contains an apertured disc carrying a fluorescent screen $F$ on the side remote from the cathode. This disc is supported from a collar $D$, sliding on the rod $E$, by a small wire which passes through a longitudinal slot in the cylinder. The position of the screen can be varied by moving the slider $D$, by means of an external magnetic field. The image is observed through the electrostatic field of the mirror by means of a telescope $T$ outside the evacuated envelope. A final cylinder $V_2$ is maintained at a potential negative to the cathode, and it is the field between this cylinder and the preceding one that constitutes the electron mirror. The whole apparatus is aligned accurately and supported inside a glass tube by means of a brass end plate carrying the leads to the electrodes. This is in contact with a ground flange on the tube, and the joint is made vacuum tight with Apiezon sealing compound $Q$. The other end is closed by a flat glass plate on a ground glass flange and the image is observed through the

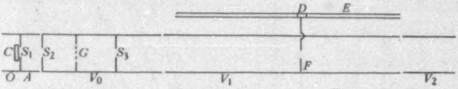


Figure 3.

plate. The vacuum is maintained by a two-stage mercury diffusion pump backed by a rotary oil pump.

The procedure for observing a reflected image is as follows. The position of the screen $F$ is fixed, giving one value of $q$, and $V_2$ is set at some definite potential negative to the cathode, while $V_1$ is set at a definite potential positive to the cathode. The voltage $V_0$ is now adjusted until an image of the grid $G$ is obtained on the screen. From the value of $V_1/V_0$, and the known object-image relations of the electrostatic lens for this potential ratio, the value of $p$ (figure 2) can be found. Thus $p$ and $q$ are obtained for a given mirror of voltage ratio $V_2/V_1$ and the overall magnification is obtained by direct measurement, and hence the mirror magnification alone can be obtained. The measurements were repeated for the complete range of values of $V_2/V_1$ giving $p$ and $m$ as a function of $V_2/V_1$ for a given $q$. After this the value of $q$ was changed and a complete series of readings of $p$ and $m$ and $V_2/V_1$ were obtained for the new value of $q$.

## §4. RESULTS FOR THE ELECTRON MIRROR

Figures 4, 5 and 6 are curves for the various quantities measured. For convenience the position of the zero equipotential surface in terms of percentage of $(V_1 - V_2)$ is given, as well as the voltage ratio $V_2/V_1$ at which the image is observed.

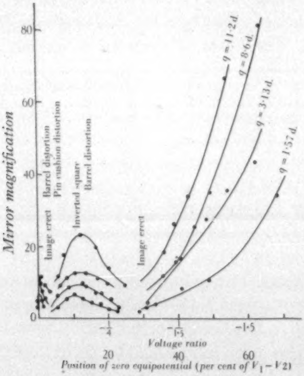


Figure 4. Mirror magnification for various values of $q$ in terms of the tube-diameter $d$.

Figure 4 shows mirror magnification $m$ as a function of $V_2/V_1$ for four values of $q$. For the two higher values of $q$, corresponding values of $p$ could not be obtained, and hence the values of $m$ for these two curves are only approximate. As in the case of electron lenses the values of $q$ are given in terms of the cylinder diameter, and this is also the case with $p$ in figures 5 and 6. The two graphs in figures 5 and 6 give $p$ as a function of the voltage ratio $V_2/V_1$, when $q = 1{\cdot}57$ tube-diameters and $q = 3{\cdot}14$ tube-diameters respectively. When the value of $V_2/V_1$ is between $-3$ and

$-1/1\cdot5$, the virtual mirror object is quite near the centre of symmetry of the mirror, which probably accounts for the high magnification and high divergence of such a system. As $V_2/V_1$ decreases to $-1/2\cdot5$, the distance of the mirror object from the centre of the mirror increases to infinity, thus accounting for the decreasing magnification and final cross-over of the beam on the screen. After this first cross-over the mirror object is again near the centre of the mirror, and as $V_2/V_1$ decreases to $-1/20$, where the second cross-over occurs on the screen, the mirror object again moves off to infinity. This process is repeated as the beam crosses over more and more times, until finally space charge, in the low-velocity region of the mirror, and increased aberrations prevent the formation of a well-defined image.

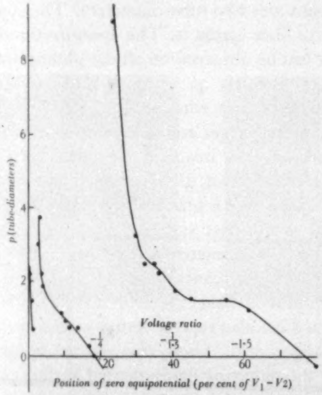


Figure 5. $q=1\cdot57$ tube-diameters.

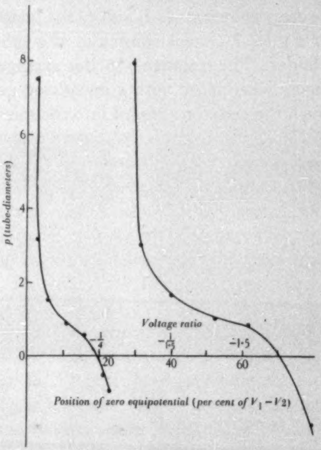
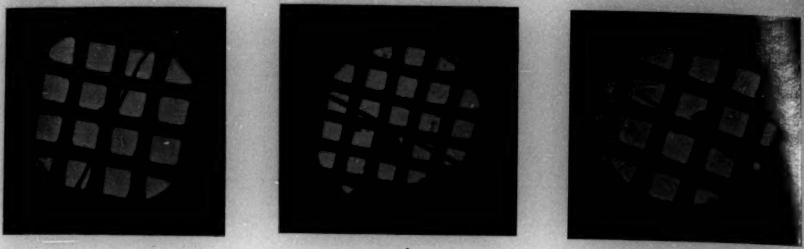


Figure 6. $q=3\cdot14$ tube-diameters.

The quality of the image was in general very good and its characteristics for various values of $V_2/V_1$ have been noted in figure 4. For voltage ratios between $-4$ and $-1\cdot5$ the image was highly magnified and erect in relation to the virtual object $B$, figure 2. As $V_2/V_1$ goes from $-1\cdot5$ to $-1/2$ the magnification decreases gradually until at about $V_2/V_1=-1/2$ the system changes from a diverging to a converging one and the image on the screen now becomes inverted in relation to $B$, and is quite sharp. In the region around $V_2/V_1=-1/4$ the image of the grid suffers from barrel distortion. On passing to $V_2/V_1=-1/10$ the image is practically square, and on alteration of $V_2/V_1$ towards $-1/20$ the image becomes pincushion-shaped. Between $V_2/V_1=-1/4$ and $V_2/V_1=-1/24$, the magnification increases to a maximum at $V_2/V_1=-1/10$ and then decreases. As $V_2$ continues towards zero a first cross-over is formed in the mirror system at $V_2/V_1=-1/40$, and one intermediate image is formed, while the image on the screen now becomes erect again and suffers from barrel distortion. As $V_2/V_1$ changes to $-1/100$ the image becomes square and at $-1/200$ it becomes pincushion-shaped. Again, at $V_2/V_1 = -1/400$ a second cross-over is formed in the field and the new image is inverted. This process continues and more and more cross-overs and intermediate images are formed as $V_2/V_1$ is brought to zero. The images after two cross-overs are, however, very badly distorted. The images of the cross wires tend to converge towards the centre of the field, and as more cross-overs are produced the image becomes unrecognizable and consists merely of bright streamers crossing the centre of the field. The above arrangement did not lend itself to photography of the image owing to the length of the second cylinder and inconvenience occasioned by the movable screen, but photographs were obtained with other two-cylinder systems which behaved in a similar manner. Figure 7 shows at *A*, *B* and *C* the image obtained for three different voltage ratios for a mirror system consisting of a cylinder $1\frac{1}{2}$ in. in diameter followed by a $\frac{1}{2}$-in. cylinder. The distance $q$ in this arrangement was two tube-diameters. The object was a nickel gauze with a metallized quartz fibre across it. The aperture through which the electrons passed into the mirror can be observed on all the photographs. The magnification of the system was approximately 40. Figure 7 A shows the image obtained at a voltage ratio $V_2/V_1$ equal to $-1/2{\cdot}7$, while in B, $V_2/V_1 = -1/17{\cdot}2$ and in C, $V_2/V_1 = -1/38{\cdot}4$. As can be seen, the images are very sharp and clear.

Figure 8 illustrates the change in the shape of the image in the region between $V_2/V_1 = -1/200$ for the case of two equal cylinders of diameter $1\frac{1}{2}$ in. A shows slight barrel distortion at $V_2/V_1 = -1/66{\cdot}6$. B shows a practically square image at an intermediate ratio of $-1/100$, and C shows an image suffering from pincushion distortion at $V_2/V_1 = -1/182$. This change in the character of the image is typical of two-cylinder mirrors when they are behaving as convergent systems, and the cycle is repeated each time a cross-over is produced inside the field of the cylinders. Thus the photographs A, B and C of figure 8 are also representative of the images obtained between voltage ratios of $-1/4$ and $-1/20$. These changes are brought about by the variation in the curvature of the reflecting equipotential surface produced by altering $V_2/V_1$, and also by the fact that the electron beam only passes through a selected portion of the field. Both of these effects are peculiar to electron mirrors and cannot be reproduced in lenses.

An interesting example of multiple reflections between the electron mirror and the zero equipotential surface near the cathode is shown in figure 9 A. Five separate images are visible on the screen. One of these images is formed each time electrons pass through the gauze towards the electron mirror and are reflected. A portion of the beam forming the first image from the mirror returns through the hole in the fluorescent screen to the cathode where it is reflected back to the mirror, which produces a second image of the gauze on the screen. Again part of the electron beam passes through to the cathode and, returning, produces a third image. This process continues and more images are formed, but the intensity steadily decreases owing to the fact that a large fraction of the beam is intercepted at the apertures for each reflection. As can be seen in the photograph, the images obtained are not superimposed; this is due to slight misalignment of the system. The superimposition of



A

$V_2/V_1 = -1/2\cdot7$

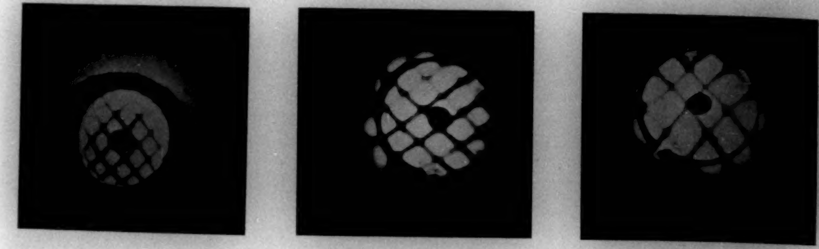
B

$V_2/V_1 = -1/17\cdot2$

C

$V_2/V_1 = -1/38\cdot4$

Figure 7.



A

$V_2/V_1 = -1/66\cdot6$

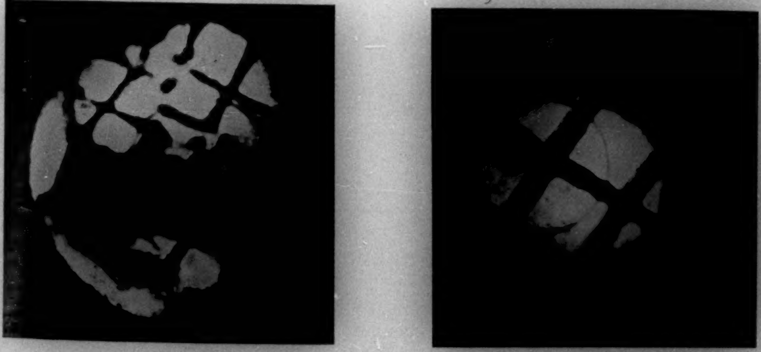
B

$V_2/V_1 = -1/100$

C

$V_2/V_1 = -1/182$

Figure 8.



A

$V_2/V_1 = -1\cdot35$

B

$V_2/V_1 = -1\cdot1$

Figure 9.

these multiple images is really an extremely critical test of alignment, and is similar to the optical method of testing compound lenses for alignment by means of reflections from the individual lens surfaces. The field and space-charge conditions of the cathode for multiple reflections are rather critical, and in the case described the cathode heater was reduced from 4 to 2·5 v. while the aperture $S_1$ and the cathode screen $A$ were maintained at +9 v. The ratio $V_2/V_1$ was $-1{\cdot}35$ and the mirror consisted of a $1\frac{1}{2}$-in. cylinder followed by a 1-in. cylinder. The result is undoubtedly dependent also on the cathode spacing behind the aperture $S_1$. In this case it was 0·3 mm. for an aperture of diameter 2 mm. The multiple reflections did not occur under the conditions for obtaining single reflections. Figure 9 B shows an image produced by a single reflection with the same two cylinders, but with the cathode screen at the same potential as the cathode and the cathode operating under space-charge conditions. As can be seen, there is no interference due to multiple reflections. The magnification is about 40 times.

## §5. BEHAVIOUR OF TWO-CYLINDER LENSES AT HIGH VOLTAGE RATIOS

The formation of intermediate images within the field of the system as described for electron mirrors has also been observed in the case of lenses of the accelerating type. The experimental arrangement was the same as in figure 3 for the mirror, but the sliding fluorescent screen $F$ was removed and another one was placed to the right of the final cylinder. The first and second cylinders of potentials $V_0$ and $V_1$ respectively were connected together and maintained at $V_1$. With this arrangement images could be obtained on the fluorescent screen at very high ratios $V_2/V_1$, of the order of 1000. They were distinct, but not of the quality of those obtained at low ratios $V_2/V_1$, although they appeared to have less pincushion distortion. It was impossible to make measurements of the focal lengths or of the intermediate image positions, but the general behaviour of the system was quite clear. Figure 10 A is an illustration of the focusing action of the electrostatic field between two cylinders at potentials $V_1$ and $V_2$ where $V_2/V_1$ is small (about 10 to 20) and the focusing is of the well known type. The lens is really compound, and is formed of a converging lens on the left and a diverging lens on the right. The converging portion of the field, however, is at the lower potential, and hence although the field is symmetrical, the converging half is stronger than the diverging half and the result is a converging system. Figure 10 B shows the same two cylinders with $V_2/V_1$ very large, of the order of 1000. Again point $A$ is reproduced at $B$, but an intermediate image is also formed at $C$ and the beam crosses over inside the lens. In this case the converging portion of the lens is so strong that the beam crosses over inside the converging field, thus producing an intermediate image. There is, however, still sufficient converging field remaining to reproduce the intermediate image at the point $B$. Figure 10 C shows the similar behaviour of a decelerating lens when $V_2/V_1$ is very small, of the order of 0·001. This effect cannot in general be observed in decelerating lenses because of the very low voltage of the final beam, but it must nevertheless occur. In this case of course the converging portion of the lens is in the second cylinder, which is now at the lower potential. If the voltage ratio $V_2/V_1$ of the two cylinders of figure 10 B is increased far beyond the value which gives one cross-over and one intermediate image, then we should expect to obtain a second cross-over and a second intermediate image. Similarly for the decelerating lens of figure 10 C we should expect to find a second cross-over and a second intermediate image when $V_2/V_1$ is made very much smaller. In both cases further cross-overs and intermediate images are to be expected as the converging field is made stronger and stronger. This behaviour is very similar to the formation of intermediate images and cross-overs in magnetic lenses with very high fields.

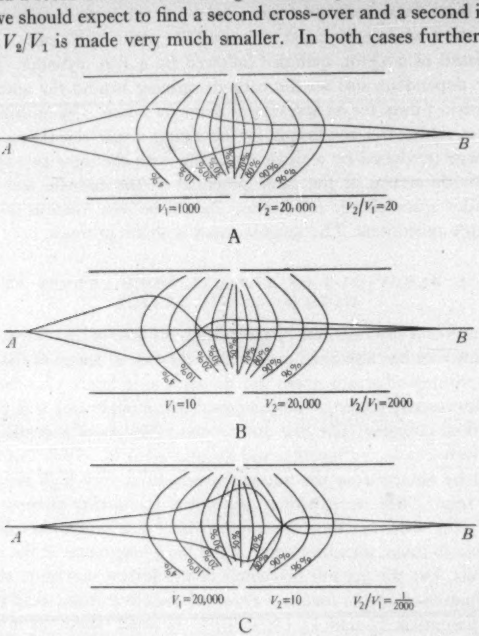


Figure 10.

## §6. GENERAL PROPERTIES OF THE TWO-CYLINDER FOCUSING SYSTEM

With the additional information which has now been obtained we can completely describe the behaviour of a two-cylinder focusing system with its corresponding optical equivalent. Figure 11 shows qualitatively the results from $V_2/V_1=-\infty$ to $V_1/V_2=+\infty$, while below are given the optical equivalents. As $V_2/V_1$ varies from $-\infty$ to zero, the mirror is at first very strongly divergent. As $-1$ is approached the system becomes less and less divergent until $V_2/V_1$ is approximately $-1/3$, when it changes over and the system becomes convergent in behaviour. From here until $V_2/V_1=0$ the convergent portion of the field becomes stronger and stronger, with the result that when $V_2/V_1$ is about $-1/40$ the first cross-over occurs inside the field and one intermediate image is formed. Again when $V_2/V_1=-1/400$ a second cross-over occurs and a second intermediate image is formed. The number of cross-overs and intermediate images will continue to increase indefinitely as zero is approached. When $V_2/V_1=0$ the system also changes over from a mirror to a converging decelerating system. As in the case of the mirror, the beam crosses over a large number of times when $V_2/V_1$ is near 0, while as $V_2/V_1$ increases towards 1 the number of cross-overs and intermediate images decreases and finally the lens behaves as a simple thick lens with no intermediate image. When $V_2/V_1=1$ the

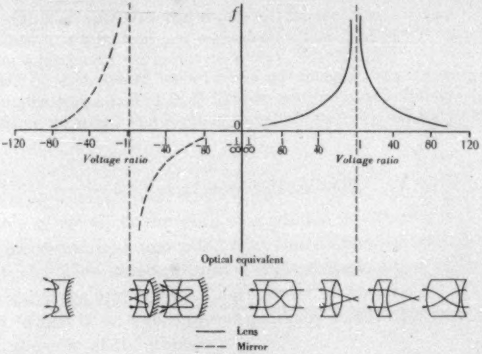


Figure 11.

focal length of the lens is infinite, and it neither accelerates nor decelerates the electron beam. As $V_2/V_1$ increases no cross-overs occur inside the system and the latter is a simple convergent accelerating lens, but somewhere in the neighbourhood of $V_2/V_1=1000$ the first cross-over occurs and one intermediate image is formed within the lens. For still higher values of $V_2/V_1$ the number of cross-overs and intermediate images will increase indefinitely as $V_2/V_1$ approaches infinity. The scale of figure 11 was so chosen that it was impossible to include very high voltage ratios. However, as is shown in the figure, at a voltage ratio of about 100 a parallel beam crosses over inside the lens, but it is not until much higher ratios are reached that divergent beams cross over and intermediate images are formed.

## §7. ACKNOWLEDGEMENTS

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# THE ANALOGY BETWEEN THE PHOTON AND THE ELECTRON AND THE DERIVATION OF THE QUANTUM EQUATION

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*ABSTRACT.* On the view that the theory of the electron is exactly analogous to the theory of the photon, a quantum law is derived which is shown to require the existence of the first-order equation of the quantum theory. The notation required for this purpose is developed, and it is shown to have certain advantages. The limiting character of the fundamental lengths $h/m_0c$ and $e^2/m_0c^2$ follows naturally, and the notation suggests that the latter should be regarded as a limit to the application of present physical theory.

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## § 1. INTRODUCTION

In the theory of relativity the path of a photon is described by a null geodesic in four dimensions, and associated with this path is the well known equation of the light wave. This association of a null geodesic and a wave equation is the expression of the wave and particle character of this particular phenomenon. This double aspect, which is now recognized generally in the quantum theory, was first recognized in the case of the photon.

The equation of motion of a particle under gravitational forces according to the theory of relativity is a geodesic, but it is not a null geodesic, while the equation of an electrically charged particle is not a geodesic at all.

The attempt to widen the theory of relativity in order to make the equation of a charged particle in a gravitational and electromagnetic field also a geodesic was made by Kaluza(1) nearly twenty years ago. He found that it was impossible to succeed in this attempt so long as the continuum was of four dimensions only. It was necessary to make use of a continuum of five dimensions, and this discovery indicates that a charged particle such as the electron requires five coordinates for its description. The usual space and time coordinates require to be completed by a fifth which we shall denote by $x^5$, placing the affix above the symbol to denote its contravariant character.

The difficulty of interpretation of this fifth coordinate was originally a drawback to the theory proposed by Kaluza. But this difficulty is removed when we recognize $x^5$ as a cyclic co-ordinate comparable with such a co-ordinate as $\theta$ in orbital motion. Such co-ordinates do not appear explicitly in the expression for the energy in the dynamics of systems and $x^5$ does not appear explicitly in the quantities required for the description of the motion of the electron.

With the advent of the quantum theory Klein(2) recognized that the notation of Kaluza had great advantages for the description of quantum relations. In this case $x^5$ preserved its character as a cyclic co-ordinate for it was disregarded except in the operation $\partial/\partial x^5$ which was placed equivalent to multiplication by $2\pi i m_0 c/h$, where $m_0$ denotes the rest mass of the electron. This may be interpreted by supposing that the functions containing the fifth co-ordinate do so in the factor

$$\exp\left[\frac{2\pi i m_0 c}{h}\, x^5\right].$$

In this case again the failure to recognize the character of $x^5$ was a disadvantage and we are left with little more than a convenient mode of description. But already certain advantages appear. Thus the principle of conservation of momentum generalized in the five-dimensional sense includes the three principles of conservation of mechanical momentum, of energy and of electric charge. In the same way that the theory of relativity unites the first two of these principles, the notation of Kaluza and Klein unites them all.

This notation was used in order to try to show the relation of the quantum theory to the theories of gravitation and of electromagnetism and a very striking discovery was made by J. W. Fisher(3), who showed that the path of the electron could be described as a null geodesic in five dimensions. This brings out the wave and particle aspect of the electron and makes it analogous to the photon. It suggests that the electron can be described by a set of field equations like those of the electromagnetic theory of light. This suggestion has been followed successfully and leads to the first-order equation of the quantum theory. These equations were discovered by Dirac who followed a different line of thought and we shall see later on the relation of Dirac's method to the theory now under discussion.

A more recent and very suggestive discovery is that the theory contains the principle of quantization of electric charge(4). This has brought out the character of $x^5$ as a cyclic co-ordinate associated with a constant momentum component $\Pi_5$ which must be a multiple of $m_0 c$, where $m_0$ is a constant recognized as the mass of the electron. Since $\Pi_5$ is proportional to the charge associated with the particle we deduce that the charge is a multiple of $\alpha m_0 c^2$, $\alpha c$ being the constant of proportionality between $\Pi_5$ and the charge. The constant $\alpha$ is a new constant introduced by Kaluza, which the present theory interprets as $e/m_0 c^2$, $e$ being the charge of the electron.

In the investigation of the relation of the quantum theory to the theory of relativity use was made of the idea of the parallel displacement of a vector, first introduced by Weyl. Eddington showed that Weyl's idea was capable of generalization and this was applied in the present application. But the extent of the generalization required was very slight. It was found that the quantum theory required a system of gauging such that in all displacements, which may be associated with displacements in physics, the change of length of a vector when it undergoes a parallel displacement is zero(5). This view of the nature of the natural gauge makes the principle of parallel displacement a physical one and not merely a cataloguing device as it was in the theory of Weyl.

This attempt at union of the quantum theory and other branches of physics has led to the recognition of the limitation of the theory, for it has shown that we shall get into difficulty in our space-time description in physics when we attempt to discuss problems in which lengths occur less than $\alpha e$, that is to say, less than $e^2/m_0c^2$.

We can compare this with the discovery of the limiting character of the velocity of light which resulted from the attempt to unite, by means of Lorentz's conception of the force on an electric charge, the theories of mechanics and electromagnetism, and also with the discovery of the new constant $h$ and its limiting character which resulted from the attempt to unite the theory of radiation with the classical theory.

The close analogy revealed between the photon and the electron in this way has its counterpart in the quantum theory. As an illustration of this we have only to translate the quantum condition which leads to the quantization of electric charge into the language of matrix operators.

Suppose that we have a total charge $q$, consisting of $n$ charges $e$. In this case the total momentum $\Pi_5$ is represented by $q/\alpha c$ and the integral relation is[4]

$$\int \Pi_5\, dx^5 = nh \qquad \text{......(1).}$$

The matrix form of this is

$$\Pi_5 x^5 - x^5 \Pi_5 = \frac{h}{2\pi i} \qquad \text{......(2).}$$

$\Pi_5$ and $x^5$ are now operators. We write $\Pi_5 = Ne/\alpha c$, where $N$ is an operator with proper values 0, 1, 2, etc., and, instead of $x^5$, which has a periodicity $h/m_0c$, we introduce $\omega$, which has unit periodicity, where $\frac{h}{m_0 c}\,\omega = x^5$.

Thus the relation becomes

$$N\omega - \omega N = \frac{1}{2\pi i} \qquad \text{......(3),}$$

and is recognized as a well known relation of the quantum theory. It is the custom, since $\omega$ is not observed, to replace the equation by another which can be deduced from it, namely,

$$Ne^{2\pi i\omega} - e^{2\pi i\omega} N = e^{2\pi i\omega} \qquad \text{......(4),}$$

or by

$$e^{-2\pi i\omega} N - Ne^{-2\pi i\omega} = e^{-2\pi i\omega} \qquad \text{......(5).}$$

A similar equation occurs in the quantum theory of the oscillator which forms the basis of the quantum theory of radiation.

The above equations thus show the close analogy existing between the quantum theory of systems of oscillators and systems of electrons and thus the analogy between the quantum theory of radiation and the quantum theory of electrons. A well known example in which this analogy has been worked out is in Fermi's theory of the beta-ray spectrum.

Our present purpose is to follow up the analogy in the case of the first-order equations of the quantum theory of the electron in the form discovered by Dirac. We shall discuss the nature of the fundamental matrices and introduce Dirac's equation in a simplified form. It will be found that the limitation to present quantum theory, which has been mentioned, is revealed very simply and its nature and source is explained.

## § 2. THE LINE ELEMENT OF THE FIVE-DIMENSIONAL CONTINUUM

The suggestion made by Kaluza in order to express the track of an electron as a geodesic is that the line element $d\sigma$ should be of a form such that

$$d\sigma^2 = \gamma_{\mu\nu} dx^\mu dx^\nu \qquad \text{......(6).}$$

Summation over $\mu$ and $\nu$ is to be made for the values 1 to 5. We begin with a statement of the values of the contravariant components $\gamma^{\mu\nu}$, since they have the most direct relation to the components $g^{mn}$ in the theory of relativity.

The Latin affixes $m$ and $n$ may have the values 1 to 4. The relations are

$$\gamma^{mn} = g^{mn}, \quad \gamma^{m5} = -\alpha\phi^m, \quad \gamma^{55} = \alpha^2 \phi_m \phi^m + \frac{1}{\gamma_{55}} \qquad \text{......(7).}$$

The quantities, $\phi^m$, denote components of the electromagnetic vector potential. The constant $\alpha$ is a new constant in the theory and has been interpreted in the way we have already described. In order to express the track of the electron in the form of a null geodesic we take $\gamma_{55} = 1$, $\alpha = e/m_0 c^2$.

It will be noted that for a null geodesic we can, without loss of generality divide throughout by $\gamma_{55}$ in the expression (6) and by writing $\gamma_{55} = 1$ we are merely taking the expression with a positive sign before $(dx^5)^2$ for convenience.

In association with equation (7) we have the following values for the covariant components $\gamma_{\mu\nu}$:

$$\gamma_{mn} = g_{mn} + \gamma_{55}\alpha^2 \phi_m \phi_n, \quad \gamma_{m5} = \gamma_{55} \alpha \phi_m \qquad \text{......(8).}$$

It will be noted that we can write $d\sigma^2$ in the form

$$d\sigma^2 = g_{mn} dx^m dx^n + \frac{1}{\gamma_{55}} (\gamma_{\mu 5} dx^\mu)^2 \qquad \text{......(9).}$$

This is very convenient for the examination of the relation of this continuum to that of the theory of relativity since the first term is the element $ds^2$ or $-c^2 d\tau^2$ of that theory, where $d\tau$ denotes an element of the proper time.

We shall write

$$\sqrt{\gamma_{55}}\, d\theta = \gamma_{\mu 5} dx^\mu \qquad \text{......(10),}$$

so that equation (9) becomes

$$d\sigma^2 = ds^2 + d\theta^2 \qquad \text{......(11).}$$

In the case of a null geodesic $d\sigma^2 = 0$ and $d\theta = c\, d\tau$. From equation (9) it is evident that $ds$ and $d\theta$ may be regarded as two perpendicular components of $d\sigma$.

The same process can be applied to any vector $C$ with components $C^\mu$. We can write

$$C^2 = g_{mn} C^m C^n + \frac{1}{\gamma_{55}} (\gamma_{\mu 5} C^\mu)^2 \qquad \text{......(12).}$$

This vector may be regarded as made up of a four-dimensional vector with components $A^m = C^m$, and of a component $(\gamma_{\mu 5}/\sqrt{\gamma_{55}})\, C^\mu$ normal to the four vector.

We shall describe the latter as $A_0$, so that with $A^2$ equal to $g_{mn}A^mA^n$ we have

$$C^2 = A^2 + A_0^2.$$

When such quantities as the five vector $C$ occur in physics it is the custom to disregard their five-dimensional character and to make use of $A$ and $A_0$. This leads to some inconvenience and to a loss of elegance in the notation.

It is important to avoid confusion and, although $C$ and $A$ together with $A_0$ denote the same quantity, we use a different symbol to lay stress on the fact that $C$ is a five vector while $A$ is a four vector.

By definition $A^m = C^m$ but $A_0 \neq C^5$.

## § 3. RELATION BETWEEN COVARIANT COMPONENTS

Any component $C_\mu$ of the covariant five vector is defined to be

$$C_\mu = \gamma_{\mu\nu} C^\nu,$$

and for the four vector we have

$$A_m = g_{mn} A^n.$$

We have

$$A_0 = \frac{\gamma_{\mu 5}}{\sqrt{\gamma_{55}}} C^\mu = \sqrt{\gamma_{55}}\,(\alpha\phi_m A^m + C^5) \text{ by equation (8)} \qquad \text{......(13)},$$

$$\begin{aligned} C_m &= \gamma_{m\nu} C^\nu = \gamma_{mn} C^n + \gamma_{m5} C^5 \\ &= g_{mn} A^n + \gamma_{55}\alpha\phi_m (\alpha\phi_n C^n + C^5) \text{ by equation (8)} \\ &= A_m + \sqrt{\gamma_{55}}\,\alpha\phi_m A_0 \qquad \text{......(14)}, \end{aligned}$$

$$\begin{aligned} C_5 &= \gamma_{5\mu} C^\mu = \gamma_{5m} C^m + \gamma_{55} C^5 \\ &= \sqrt{\gamma_{55}}\,A_0 \qquad \text{......(15)}. \end{aligned}$$

The element $d\theta$ already introduced is a particular case of $A_0$,

$$d\theta = \sqrt{\gamma_{55}}\,(\alpha\phi_m dx^m + dx^5).$$

## § 4. THE MATRIX LINE ELEMENT

In order to make use of geometric ideas and of a geometric mode of description in quantum mechanics it has been found convenient to introduce the matrix line element. This is defined to be$^{(6)}$

$$d\sigma = \gamma_\mu dx^\mu \qquad \text{......(16)},$$

where $\gamma_\mu$ is one of five matrices regarded as of fundamental significance. Associated with them there are contravariant matrices $\gamma^\mu$ and we connect them by means of the same relation as that which holds for vectors,

$$\gamma_\mu = \gamma_{\mu\nu}\gamma^\nu.$$

We also make use of certain relations introduced by Tetrode originally for four-dimensional matrices. These are

$$\gamma_\mu\gamma_\nu + \gamma_\nu\gamma_\mu = 2\gamma_{\mu\nu} \qquad \text{......(17)},$$

and

$$\gamma^\mu\gamma^\nu + \gamma^\nu\gamma^\mu = 2\gamma^{\mu\nu} \qquad \text{......(18)}.$$

The latter gives for the cases where $\mu$ and $\nu$ are not equal to 5

$$\gamma^m\gamma^n + \gamma^n\gamma^m = 2\gamma^{mn} = 2g^{mn} \qquad \text{......(19)}.$$

## § 5. THE MATRICES OF THE QUANTUM THEORY(7)

In the quantum theory four matrices $\alpha^m$ together with a fifth $\beta$ are introduced. We shall now describe $\beta$ by the symbol $\alpha_0$. According to Tetrode these satisfy the relations

$$\left.\begin{aligned}\alpha^m\alpha^n+\alpha^n\alpha^m&=2g^{mn}\\ \alpha_m\alpha_n+\alpha_n\alpha_m&=2g_{mn}\end{aligned}\right\} \qquad \text{......(20).}$$

In the notation we have used for the line element $g^{11}$, $g^{22}$ and $g^{33}$ are all $+1$ in the case when the gravitational field is neglected. This means that the matrices used by Dirac are equal to these $\alpha$'s.

In addition we have the relations

$$\alpha_0\alpha_m+\alpha_m\alpha_0=0 \qquad \text{......(21),}$$

$$(\alpha_0)^2=1 \qquad \text{......(22).}$$

We shall show that the matrices $\alpha^m$ and $\alpha_0$ bear the same relation to the matrices $\gamma^\mu$ as the components $A^m$ and $A_0$ bear to $C^\mu$. We have to show that the $\alpha$ matrices derived from the $\gamma$ matrices according to the following equations, satisfy equations (20), (21) and (22),

$$\alpha_0=\sqrt{\gamma_{55}}\,(\alpha\phi_m\gamma^m+\gamma^5) \qquad \text{......(23),}$$

$$\gamma_m=\alpha_m+\sqrt{\gamma_{55}}\,\alpha\phi_m\alpha_0 \qquad \text{......(24),}$$

$$\gamma_5=\sqrt{\gamma_{55}}\,\alpha_0 \qquad \text{......(25).}$$

The identification of $\gamma^m$ with $\alpha^m$ causes the first equation (20) to follow at once as a consequence of equation (19) and the definition of $\alpha_m$ as $g_{mn}\alpha^n$ means that Tetrode's second relation (20) is satisfied.

In order to verify equation (21) we note that

$$\alpha_m\alpha_0=g_{mn}\gamma^n\sqrt{\gamma_{55}}\,(\alpha\phi_l\gamma^l+\gamma^5)=\sqrt{\gamma_{55}}\,(\alpha g_{mn}\phi_l\gamma^n\gamma^l+g_{mn}\gamma^n\gamma^5).$$

Thus $$\alpha_m\alpha_0+\alpha_0\alpha_m=2\sqrt{\gamma_{55}}\,(\alpha g_{mn}\phi_l g^{nl}-\alpha g_{mn}\phi^n)=0.$$

Finally from equation (25) $$(\gamma_5)^2=\gamma_{55}\alpha_0{}^2.$$

But since $(\gamma_5)^2=\gamma_{55}$ it follows that $\alpha_0{}^2=1$.

The matrix line element may now be expressed in the form

$$d\sigma=\alpha_m dx^m+\alpha_0 d\theta \qquad \text{......(26),}$$

and corresponding to any vector $A$ we have a vector matrix $(\alpha_m A^m+\alpha_0 A_0)$. It is evident that the square of the vector matrix is equal to the square of the vector.

## § 6. THE QUANTUM EQUATION(8)

In the theory of general relativity the law of gravitation is expressed by a relation between the components $g_{mn}$. The law is arrived at by means of the Riemann Christoffel tensor $B^p{}_{lmn}$. The vanishing of this quantity indicates that the geometry of space-time is Galilean, and a law such as $B^p{}_{lmn}=0$ is too stringent for the physical world. The law adopted by Einstein is one affecting the contracted tensor $G_{lm}$, equal to $B^n{}_{lmn}$. Originally the law was $G_{lm}=0$ but later a modification $G_{lm}=\lambda g_{lm}$ was adopted.

It may be expected that a relation of a similar character exists between the fundamental matrices, and it has been shown that the quantum equation of an electron in an electromagnetic field is actually this relation. The form taken by the relation in terms of the matrices $\gamma^\mu$ is simpler and its derivation easier than is the case in which the matrices $\alpha^m$ and $\alpha_0$ are used. In order to derive the relation we shall make use of Weyl's conception of gauging and apply it to the matrix length. It will be remembered that Weyl modified the scheme of Riemannian geometry by supposing that when a vector was subjected to a parallel displacement it incurred a change in length. His object was to introduce the electromagnetic vector potential into a system of geometry and metrics and so to bring the theory of electromagnetism into the scheme of relativity.

This union is achieved in five-dimensional theory by the use of the coefficients $\gamma_{\mu\nu}$ with the values (7) and (8). We may therefore suppose that no such change of length occurs when a five-dimensional vector with components $C^\mu$ undergoes a parallel displacement in the five-dimensional continuum. It may be, however, that the corresponding four-dimensional vector $A^m$, related as we have seen above to the five-dimensional vector, does undergo a change of length.

We are therefore led to assume that the rule for the change in a vector component when it is displaced in this way is

$$dC^\mu = -\Gamma_{\lambda\nu}{}^\mu C^\lambda dx^\nu \qquad \text{......(27)},$$

or in terms of the covariant component

$$dC_\mu = \Gamma_{\mu\nu}{}^\lambda C_\lambda dx^\nu \qquad \text{......(28)},$$

where the $\Gamma$'s denote components of the Christoffel bracket expression. This means that the length $C^2$, equal to $C^\mu C_\mu$, is unchanged when this displacement occurs.

We shall now investigate the change in the matrix length $\gamma^\mu C_\mu$ when $C$ is so displaced and in order to do this we shall suppose that a matrix $\psi$ exists as a function of the co-ordinates such that (6)

$$L = \gamma^\mu C_\mu \psi \qquad \text{......(29)}$$

remains unchanged when $C_\mu$ is changed according to equation (28). $\psi$ is taken to be a matrix with one column only, i.e. there are four components $\psi_{11}$, $\psi_{21}$, $\psi_{31}$ and $\psi_{41}$, which we can conveniently describe as $\psi_1$, $\psi_2$, $\psi_3$ and $\psi_4$.

The change in $L$ is

$$\frac{\partial\gamma^\mu}{\partial x^\nu} C_\mu \psi \, dx^\nu + \gamma^\mu \Gamma_{\mu\nu}{}^\lambda C_\lambda \psi \, dx^\nu + \gamma^\mu C_\mu \frac{\partial\psi}{\partial x^\nu} dx^\nu = 0 \qquad \text{......(30)}.$$

If this is to be true for all displacements and for all vectors, we require

$$\left(\frac{\partial\gamma^\mu}{\partial x^\nu} + \gamma^\rho \Gamma_{\rho\nu}{}^\mu\right) \psi + \gamma^\mu \frac{\partial\psi}{\partial x^\nu} = 0 \qquad \text{......(31)}.$$

We write this in the form (9)

$$\gamma^\mu \frac{\partial\psi}{\partial x^\nu} + K_\nu{}^\mu \psi = 0 \qquad \text{......(32)},$$

$$K_\nu{}^\mu = \frac{\partial\gamma^\mu}{\partial x^\nu} + \gamma^\rho \Gamma_{\rho\nu}{}^\mu \qquad \text{......(33)}.$$

We now suppose that a law exists, which we may call the quantum law, affecting $K_\nu{}^\mu$. But there is nothing to suggest the simple law $K_\nu{}^\mu = 0$, in fact this would require $\psi$ to be constant. This appears to be too stringent a condition for our purpose. Thus as in the case with the Riemann-Christoffel tensor we take the less stringent condition

$$K_\mu{}^\mu = 0 \qquad \text{......(34).}$$

Summation over the five values of $\mu$ is, of course, implied. This is the quantum law and leads by equation (32) to

$$\gamma^\mu \frac{\partial \psi}{\partial x^\mu} = 0, \qquad \text{......(35),}$$

which we can describe as a law of gauging since it imposes a condition upon the gauging function $\psi$.

Equation (35) when expressed in terms of the matrices $\alpha^m$ and $\alpha_0$ is seen to be Dirac's equation, so that $\psi$ is recognized as the function introduced into the quantum theory of the electron. In order to show that this is the case we shall make use of the conditions we have seen to be necessary in describing the path of the electron as a null geodesic. These are

$$\frac{\partial \psi}{\partial x^5} = \frac{2\pi i m_0 c}{h} \psi,$$

and

$$\alpha = e/m_0 c^2.$$

We have according to our notation

$$\gamma^m = \alpha^m \quad \text{and} \quad \gamma^5 = \alpha_0 - \alpha \phi_m \alpha^m.$$

Thus equation (35) becomes

$$\alpha^m \left( \frac{h}{2\pi i} \frac{\partial}{\partial x^m} - \frac{e}{c} \phi_m \right) \psi + m_0 c \alpha_0 \psi = 0 \qquad \text{......(36),}$$

which is the first-order equation of the quantum theory.

## § 7. THE VALUE OF $K_\nu{}^\mu$

We cannot actually derive the value of $K_\nu{}^\mu$ but we can see the form it takes by means of Tetrode's relations and by means of the identity

$$\frac{\partial \gamma^{\mu\nu}}{\partial x^\sigma} + \Gamma_{\alpha\sigma}{}^\mu \gamma^{\sigma\nu} + \Gamma_{\alpha\sigma}{}^\nu \gamma^{\sigma\mu} = 0.$$

Combining this with equation (18) we obtain

$$(K_\sigma{}^\mu \gamma^\nu + \gamma^\nu K_\sigma{}^\mu) + (K_\sigma{}^\nu \gamma^\mu + \gamma^\mu K_\sigma{}^\nu) = 0.$$

It is clear that if $K_\sigma{}^\mu$ is of the form

$$K_\sigma{}^\mu = \Gamma_\sigma \gamma^\mu - \gamma^\mu \Gamma_\sigma \qquad \text{......(37),}$$

this relation is satisfied identically. $\Gamma_\sigma$ is an undetermined matrix. Thus

$$\frac{\partial \gamma^\mu}{\partial x^\nu} + \gamma^\rho \Gamma_{\rho\nu}{}^\mu = \Gamma_\nu \gamma^\mu - \gamma^\mu \Gamma_\nu \qquad \text{......(38).}$$

This may be regarded as defining the operation of differentiation of the matrix $\gamma^\mu$ with respect to $x^\nu$.

In the case of the first four components $\gamma^m$ or $\alpha^m$ and for differentiation with respect to any variable $x^n$ we have

$$\frac{\partial\alpha^m}{\partial x^n}+\alpha^r\Gamma_{rn}{}^m+\gamma^5\Gamma_{5n}{}^m=\Gamma_n\alpha^m-\alpha^m\Gamma_n \qquad \text{......(39).}$$

It must be remembered that $\Gamma_{rn}{}^m$ has to be calculated for the five-dimensional continuum and that it is not equal to the corresponding quantity in four dimensions, viz. $\begin{Bmatrix} rn \\ m \end{Bmatrix}$.

The equation (39) suggests the four-dimensional form

$$\frac{\partial\alpha^m}{\partial x^n}+\begin{Bmatrix} rn \\ m \end{Bmatrix}\alpha^r=\Gamma_n\alpha^m-\alpha^m\Gamma_n \qquad \text{......(40).}$$

Equations (39) and (40) are not identical, but they differ only by a term multiplied by the factor $e/m_0c^2$, which makes the difference small(10).

It is thus natural to adopt the form (40) for the $\alpha^m$. When the gravitational field exerts no influence upon the phenomenon considered, as is always the case in quantum problems, equation (40) becomes

$$\frac{\partial\alpha^m}{\partial x^n}=\Gamma_n\alpha^m-\alpha^m\Gamma_n \qquad \text{......(41).}$$

In the particular case when $x^n$ is the fourth co-ordinate ($x^4=ict$), equation (41) takes the form

$$\frac{\partial\alpha^m}{\partial t}=\frac{2\pi i}{h}(H\alpha^m-\alpha^m H) \qquad \text{......(42),}$$

where $(2\pi i/h)\,H$ denotes $ic\Gamma_4$.

It will be recognized that this is the equation adopted in the quantum theory for the differentiation of an operator with respect to the time, $H$ being the quantum Hamiltonian operator. The treatment here may be regarded as providing the derivation of this differentiation in the case of the matrix operators $\alpha^m$.

From equation (42) we adopt the suggestion(11) that for any operator $Q$

$$\frac{dQ}{dt}=\frac{\partial Q}{\partial t}+\frac{2\pi i}{h}(HQ-QH) \qquad \text{......(43),}$$

the partial differentiation with respect to $Q$ being included on the right-hand side since, in the general case, an operator may contain the time explicitly in its components.

## § 8. CHANGE OF LENGTH WITH PARALLEL DISPLACEMENT

The foregoing treatment is based on the assumption that the length of a vector $C$ is unchanged when it undergoes a parallel displacement. But since $C^2=A^2+A_0{}^2$, it does not follow that $A$ undergoes no change when it is subject to a parallel displacement in four dimensions.

It has been deduced from the idea that the quantum equations indicate a modification in Weyl's gauging law that the change of length in $A$ is given by⁽¹²⁾

$$\frac{dA^2}{A^2} = -\frac{2\pi i m_0 c}{h}\, dx^5.$$

Thus the change depends only upon changes in $dx^5$.

These considerations have also suggested that $dx^5$ changes by amounts which are integral multiples of $h/m_0 c$ in those cases which have a physical significance. Thus in the physical world the displacements are such that the change of length is zero. It may be that this is the significance of the cyclic character of $x^5$. It follows that a limitation exists in our description of the physical world which is expressed by

$$dx^5 \nless h/m_0 c \qquad \text{......(44).}$$

We do not make use of $x^5$ explicitly so that we must make use of a substitution provided by

$$d\theta = \sqrt{\gamma_{55}}\,(\alpha\phi_m dx^m + dx^5) \qquad \text{......(45).}$$

If we apply this to the track of a charge $e$ with $d\theta = c\,d\tau$ and $\gamma_{55} = 1$, we have

$$dx^5 = c\,d\tau - \alpha\phi_m dx^m.$$

Thus the limitation (44) becomes

$$c\,d\tau - \alpha\phi_m dx^m \nless h/m_0 c \qquad \text{......(46),}$$

$$m_0 c^2 d\tau - \frac{e}{c}\,\phi_m dx^m \nless h.$$

The significance of this has already been discussed⁽¹³⁾. It means that in any case where $(e/c)\,\phi_m dx^m$ is small, $d\tau \nless h/m_0 c^2$. This condition holds approximately for charges moving in orbits of light atoms where $\phi_1$, $\phi_2$ and $\phi_3$ are zero and $\phi_4 = Ne/r$, $Ne$ denoting the nuclear charge. In this case the value of $r$ is sufficiently great to cause the second term to be neglected. We deduce that in the orbits no element of time less than $h/m_0 c^2$ is observable⁽¹⁴⁾. But the case is different where two charges $e$ of opposite sign approach one another to a distance of the order of $e^2/m_0 c^2$. Then, supposing for the sake of illustration that the velocities of the charges are well below that of light so that we can regard $d\tau$ as an interval of ordinary time, the formula gives a very large interval for the smallest element of time $dt$ which we can introduce into our equations. For the case of charges approaching one another within this distance our conception of localization in time breaks down. This is the limitation which we mentioned above as resulting from the attempt to study the relation of the quantum theory to the theory of electromagnetism and to the principle of relativity. The result has been to show the limiting character of the length $e^2/m_0 c^2$. It would appear reasonable to conclude that we shall find difficulties in our theory when we attempt to consider lengths of this order of magnitude⁽¹⁵⁾.

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# A NOTE ON THE NEAR ULTRA-VIOLET BAND SYSTEM OF SnO

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*ABSTRACT.* The note refers to Connelly's paper on SnO and Jevons, Bashford and Briscoe's paper on GeO, which appeared in these *Proceedings* in 1933 and 1937 respectively. In the latter it was pointed out that, judged by the data for related band systems of the monoxides of group-IV(*b*) elements, Connelly's values of the anharmonic coefficients $x_e \omega_e$ were anomalous, the value 3·6 cm.$^{-1}$ for the ground state being slightly low, and the value 1·1 cm.$^{-1}$ for the excited state being much too low. As a result of a re-examination of Connelly's band-head data the expression

$$\nu_{\text{head}} = 29624{\cdot}9 + (582{\cdot}6u' - 3{\cdot}08u'^2 - 0{\cdot}135u'^3) - (822{\cdot}4u'' - 3{\cdot}73u''^2)$$

is now proposed for the A ⇄ X system, and the anomalies are thereby removed.

So far as it has been investigated the spectrum of SnO consists of three band systems B → X, A ⇄ X and D → X, which are analogous to the SiS systems A ← X, B ← X and C ← X respectively$^{(1)}$, and a few bands attributed to a fourth system C → X$^{(2)}$, which appears to have no counterpart in SiS. In every case the bands degrade towards the red. The prominent SnO system A ⇄ X, known in various flame, arc and spark sources for more than half a century, has been observed by Connelly$^{(3)}$ both in emission from λ3071 to λ4488 by a high-voltage a.c. discharge through a flame of hydrogen charged with $SnCl_4$ vapour, and also in absorption by the flame itself. A recent rotational analysis of three bands of this system by Mahanti and Sen Gupta$^{(4)}$ indicates an electronic transition $A^1\Sigma \rightleftarrows X^1\Sigma$. The less extensive systems (or fragments of systems) B → X and C → X consist of about 20 bands in the region λ3854–λ4569, overlapping the visible end of A ⇄ X;* bands of these were recorded, together with A → X, by the earlier investigators, notably Eder and Valenta and von der Seipen, and have been observed more recently by Mahanti$^{(2)}$ in the arc in air, by Connelly in the discharge through flame and by the present writer$^{(5)}$ in an uncondensed discharge through a mixture of $O_2$ and $SiCl_4$ vapour in a discharge tube. The more refrangible system D → X was discovered by Loomis and Watson$^{(6)}$ in the region λ2390–λ3090 in a tin arc in oxygen at reduced pressure. It is with the main system A ⇄ X that this note is concerned.

* Mahanti's$^{(2)}$ interpretation of the common lower state of what are now called A ⇄ X and B → X as an excited state of SnO, the lower state of C → X being the ground state, was, of course, disproved by Connelly's observation of A ⇄ X in absorption$^{(3)}$, and is implicitly withdrawn in Mahanti and Sen Gupta's recent paper$^{(4)}$.

In a recent paper in these *Proceedings*[(7)] on the ultra-violet system of GeO, a graphical study was made of the vibrational coefficients of both upper and lower states in the band systems of the monoxides and monosulphides of the group-IV(*b*) elements which appeared to be analogous to the fourth positive system of CO and the well known ultra-violet systems of CS and SiO. For SnO the extensive system $A \rightleftarrows X$ was, naturally, chosen for this study and Connelly's expression[(3)],

$$\nu_{\text{head}} = 29626{\cdot}1 + (578{\cdot}9u' - 1{\cdot}1u'^2 - 0{\cdot}32u'^3) - (821{\cdot}9u'' - 3{\cdot}6u''^2)$$

was accepted ($u$ is written here for $v+\frac{1}{2}$). This expression differs considerably, especially in regard to the upper state, from Mahanti's expression for the same system, namely

$$\nu_{\text{head}} = 29630{\cdot}5 + (586{\cdot}0u' - 6{\cdot}0u'^2) - (824{\cdot}0u'' - 4{\cdot}0u''^2),$$

although the assignments of $v'$ and $v''$ to the bands observed by both are the same in the two cases. The discrepancy is due to the fact that Connelly's observations, extending to $v'=8$, require a cubic term for the higher levels and also bring to light very large perturbations of rotational levels associated with the vibrational level $v'=3$, whereas Mahanti's observations, which end with $v'=3$, neither require a cubic term nor reveal the perturbations.

It was found[(7)] that, when the vibrational coefficients $\omega_e$ and $x_e\omega_e$ are plotted against the number of electrons in the molecule, two pairs of uninflected curves can be drawn for the ground states of the monoxides and the monosulphides (figure 3 in the GeO paper[(7)]), and two similar pairs for the excited states (figure 4[(7)]). In the case of SnO, the first coefficients $\omega_e''$ and $\omega_e'$ fall exactly on their respective curves, and the second coefficient $x_e''\omega_e''$ for the ground state (3·6 cm.$^{-1}$) also falls on its curve although it gives the oxide curve a minimum for which there is no counterpart in the corresponding sulphide curve, but Connelly's value (1·1 cm.$^{-1}$) of the second coefficient $x_e'\omega_e'$ for the upper state is much too low to fall on any simple curve passing through the $x_e'\omega_e'$ values for the other monoxides. The difficulty is not to be overcome by discarding Connelly's coefficients in favour of Mahanti's, for, according to the graphs, while Mahanti's value (4·0 cm.$^{-1}$) of $x_e''\omega_e''$ is entirely satisfactory, his value (6·0 cm.$^{-1}$) of $x_e'\omega_e'$ is about as much too large as Connelly's (1·1 cm.$^{-1}$) is too small.

As Connelly's band-head measurements are the more extensive it has been thought desirable to see whether they can be satisfactorily represented by any other expression than that quoted above, the assignments of $v'$ and $v''$ remaining unchanged. Both the wave-numbers corresponding to the measured wave-lengths in Connelly's table 1 and also the vibrational intervals in his table 2 have been checked.* Those intervals that involve comparatively rough measures of band-heads (shown in parentheses in Connelly's table ) have been ignored entirely. The means of the others are as follows:

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* For the 7, 2 band either $\lambda$ or $\nu$ is given wrongly. It has been assumed that $\nu$ 31738·3 is correct and that $\lambda$ should be 3149·86 instead of 3149·16.

Excited state

| $v'$ | $\frac{1}{2}$ | $1\frac{1}{2}$ | $2\frac{1}{2}$ | $3\frac{1}{2}$ | $4\frac{1}{2}$ | $5\frac{1}{2}$ | $6\frac{1}{2}$ | $7\frac{1}{2}$ |
|---|---|---|---|---|---|---|---|---|
| $\Delta G'(v')$ | 575·2 | 563·6 | 541·9 | 583·6 | 541·1 | 532·7 | 517·1 | 502·9* |

Ground state

| $v''$ | $\frac{1}{2}$ | $1\frac{1}{2}$ | $2\frac{1}{2}$ | $3\frac{1}{2}$ | $4\frac{1}{2}$ | $5\frac{1}{2}$ | $6\frac{1}{2}$ | $7\frac{1}{2}$ | $8\frac{1}{2}$ |
|---|---|---|---|---|---|---|---|---|---|
| $\Delta G''(v'')$ | 814·9 | 806·1† | 800·6 | 793·7 | 784·7 | 776·5‡ | 772·9 | 764·4 | 752·4 |

* $\Delta G'(7\frac{1}{2})$ must be discarded since its value depends entirely on measurements of the 7, 4 and 8, 4 band-heads, and the latter is superposed on the 2, 0.

† Value of $\Delta G''(1\frac{1}{2})$ obtained after correction of the interval between the 5, 1 and 5, 2 band-heads from 806·3 to 804·3.

‡ Value of $\Delta G''(5\frac{1}{2})$ obtained after correction of the interval between the 1, 5 and 1, 6 band-heads from 783·1 to 777·1.

These have been used in a graphical determination of the coefficients.

The ground state presents no difficulty whatever. In the excited state, however, the intervals $\Delta G'(1\frac{1}{2})$, $\Delta G'(2\frac{1}{2})$ and $\Delta G'(3\frac{1}{2})$ are affected, as Connelly's figure 3 clearly shows, not only by the very large perturbations at $v'=3$, but also by smaller perturbations at $v'=2$ not mentioned by Connelly, and the single observation of $\Delta G'(7\frac{1}{2})$ is not entirely satisfactory. Only the intervals $\Delta G'(\frac{1}{2})$, $\Delta G'(4\frac{1}{2})$, $\Delta G'(5\frac{1}{2})$ and $\Delta G'(6\frac{1}{2})$, therefore, can be used in the derivation of the coefficients of $u'$, $u'^2$ and $u'^3$.

In the absence of a term in $u'^3$ the value of

$$\tfrac{1}{8}\{\Delta G'(\tfrac{1}{2})-\Delta G'(4\tfrac{1}{2})\},\ \text{namely}\ \tfrac{1}{8}\{575{\cdot}2-541{\cdot}1\}\ \text{or}\ 4{\cdot}26\ \text{cm.}^{-1},$$

would, of course, be a good estimate of $x_e'\omega_e'$, and as such it would be not much too large in comparison with the values of $x_e'\omega_e'$ for GeO and PbO, namely 4·24 and 2·33 cm.$^{-1}$ respectively; it would, indeed, be much better than either Connelly's 1·1 or Mahanti's 6·0. A term in $u'^3$ is necessary, but it is unlikely to be so large as to reduce $x_e'\omega_e'$ from about 4·26 to 1·1. It is reasonable, then, to seek an expression with a much larger coefficient of $u'^2$, and a much smaller one of $u'^3$, to represent the unperturbed band-heads. Such an expression has been obtained, namely

$$\nu_{\text{head}}=29624{\cdot}9+(582{\cdot}6u'-3{\cdot}08u'^2-0{\cdot}135u'^3)-(822{\cdot}4u''-3{\cdot}73u''^3).$$

In view of the fewness and irregularity of the available data no accuracy is claimed for the last figure in any of the coefficients. The residuals ($\nu_{\text{obs.}}-\nu_{\text{calc.}}$) are so small that no importance need be attached to the anomaly presented by Connelly's expression. As $\omega_e'$ for this state of SnO, Howell⁽¹⁾ gives 582 cm.$^{-1}$, which is in fair agreement with the value now proposed, but he does not state whence it is obtained, or publish his own SnO absorption data.

In figures 3 and 4 of the GeO paper⁽⁷⁾, the PbO coefficients used were those of Shawhan and Morgan's⁽⁸⁾ expression for the B ⇄ X system, namely

$$\nu_{\text{head}}=22889{\cdot}9+(496{\cdot}6u'-2{\cdot}33u'^2)-(722{\cdot}5u''-3{\cdot}75u''^2);$$

unfortunately Howell's expression for the same system⁽¹⁾,

$$\nu_{\text{head}}=22884{\cdot}9+(498{\cdot}0u'-2{\cdot}20u'^2)-(721{\cdot}8u'-3{\cdot}70u''^2),$$

was overlooked. If the latter expression and that now proposed for SnO be adopted, both $x_e'\omega_e'$ and $x_e''\omega_e''$ diminish from GeO to PbO, just as they do from GeS to PbS⁽⁷⁾, thus:

| | GeO | SnO | PbO |
|---|---|---|---|
| $x_e'\omega_e'$ | 4·24 | 3·08 | 2·20 |
| $x_e''\omega_e''$ | 4·30 | 3·73 | 3·70 |

The SnO value now falls on a smooth $x_e'\omega_e'$ curve (figure 4⁽⁷⁾) and the $x_e''\omega_e''$ curve for the oxides (figure 4⁽⁷⁾) no longer has a minimum at SnO. The slight changes in the coefficients of $u'$ and $u''$ have no effects on the $\omega_e'$ and $\omega_e''$ curves for the oxides.

In a forthcoming paper⁽⁹⁾ on the recently discovered band spectrum of SiS, the reciprocals of the coefficients, instead of the coefficients themselves, are plotted against the number of electrons, in order that graphs of simpler form and smaller curvature may be obtained, and irregularities in the values of the coefficients made more evident. The values now proposed for SnO and PbO are far more satisfactory than those used previously, and it would appear that any further improvement for SnO can only be obtained from entirely new observational data.

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# NOTE ON THE USE OF PROBABILITY PAPER

By E. W. H. SELWYN

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*Read in title* 14 *October* 1938

*ABSTRACT*. The method of plotting observations on probability paper becomes clear when the curve on probability paper is regarded as an integral of the histogram. The observations are divided into intervals, which may often conveniently be the natural interval of measurement, and the total frequency of observations up to and including the *n*th interval is plotted against the upper end of the *n*th interval. It is shown that in drawing a straight line to fit the observations, less weight should be attributed to deviations from the line towards its ends than in the middle regions.

## § 1. INTRODUCTION

During the course of an investigation into the nature of the graininess of photographic materials, a considerable amount of data was secured concerning the statistical variation of density over the area of the materials. The time required for the calculation of the mean distribution curve, which was expected to be Gaussian, by the usual methods was considered prohibitive, and recourse was had to probability paper(1, 2). It was found that there was some disagreement concerning the proper use of this method of representing statistical data, and it is the purpose of the present note to put forward what is believed to be the correct method.

Probability paper is printed with a uniform scale of abscissae, but with ordinates so scaled that if the percentage of observations which have values up to a certain figure is plotted as ordinate against that figure as abscissa for a series of values, a straight line can be drawn through the plotted points, when the distribution is Gaussian.

## § 2. AN EXAMPLE

We shall take an example from the investigation mentioned above. Table 1 shows the percentage of occurrences of certain densities in a sample of photographic material. The densities are measured from an arbitrary zero.

The densities were read to 0·001: in other words, it is assumed that a density labelled 0·028 may in fact lie anywhere between 0·0275 and 0·0285. Thus a histogram with 0·001 as the interval may be constructed directly from the observations, as shown in figure 1 A. The curve to be obtained on probability paper is then the integral of this histogram, but drawn with suitably distorted ordinates. Therefore, the total percentage of observations up to and including a nominal density of 0·028 should be plotted at 0·0285 and the total percentage recorded up to 0·029 should be plotted at 0·0295, and so on. The only reasonable representation of the course of the curve joining these two points, or any other similar pair, is a straight line, corresponding with integration of the histogram. Presumably this line should be straight for an integral drawn with a uniform scale of frequency, and therefore curved when drawn on probability paper. However, as the difference is not great the points on probability paper have been joined by straight lines in figure 1 B. It will be noted that the two end points, 0 and 100 per cent, are off at infinity, owing to the nature of the scale of ordinates. It is probably the omission of these points, in their correct places on the scale of abscissae, that has led to the confusion already mentioned concerning the proper method of plotting. If the points are plotted in a way different from that suggested here, it will be found that if in summing the individual frequencies of table 1, for instance, we start from the low-density end of the series of observations, we obtain a curve different from that which we should obtain by summing from the high-density end and using a reversed frequency scale. This is clearly an improper result.

Table 1

| Density | Frequency (per cent) | Total frequency (per cent) |
|---|---|---|
| 0·019 | 0·5 | 0·5 |
| 0·020 | 0·5 | 1·0 |
| 0·021 | 0·0 | 1·0 |
| 0·022 | 1·5 | 2·5 |
| 0·023 | 0·5 | 3·0 |
| 0·024 | 3·0 | 6·0 |
| 0·025 | 4·5 | 10·5 |
| 0·026 | 5·0 | 15·5 |
| 0·027 | 4·0 | 19·5 |
| 0·028 | 8·5 | 28·0 |
| 0·029 | 7·5 | 35·5 |
| 0·030 | 12·5 | 48·0 |
| 0·031 | 9·0 | 57·0 |
| 0·032 | 8·0 | 65·0 |
| 0·033 | 5·5 | 70·5 |
| 0·034 | 6·5 | 77·0 |
| 0·035 | 11·0 | 88·0 |
| 0·036 | 3·0 | 91·0 |
| 0·037 | 2·5 | 93·5 |
| 0·038 | 2·0 | 95·5 |
| 0·039 | 1·5 | 97·0 |
| 0·040 | 2·0 | 99·0 |
| 0·041 | 0·5 | 99·5 |
| 0·042 | 0·5 | 100·0 |

In practice it is hardly necessary to join up the plotted points. It is desirable, however, to plot a point for every interval in the histogram. That is to say, when a value does not appear among the actual observations it should still be plotted, as at *a* in figure 1 B, with the same frequency as the preceding value. Naturally, the histogram is not drawn in practice when probability paper is used. For straightforward observations the natural interval may conveniently be adopted as in the above example. When the quantities are derived from other observations there may be no natural interval, and the observations should then be classed into intervals exactly as if the histogram were to be drawn.

## § 3. THE STRAIGHT LINE REPRESENTING THE OBSERVATIONS

In estimating the Gaussian distribution which fits an observed distribution it is usual to obtain the constants of the Gaussian distribution by making the first and second moments of the Gaussian distribution equal to the first and second moments of the observed distribution. It will be supposed that the straight line should be drawn so as to fulfil these two conditions. As may readily be seen by reference to figures 1A and 1B, if $\Delta f_n$ is the deviation of the observed frequency from the frequency indicated by the straight line at $x_n$, the point dividing the $n$th interval from the $(n+1)$th, equality of the first moments is obtained when*

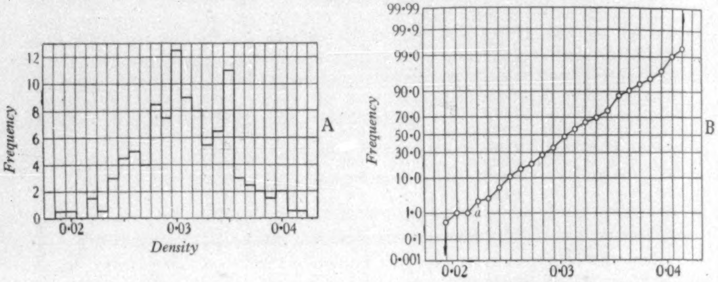

Figure 1. Relation between the histogram A and the curve on probability paper B.

$$\Sigma\left(\frac{x_n+x_{n+1}}{2}-x_0\right)(\Delta f_{n+1}-\Delta f_n)=0$$

and equality of the second moments when

$$\Sigma\left(\frac{x_n+x_{n+1}}{2}-x_0\right)^2(\Delta f_{n+1}-\Delta f_n)=0.$$

These conditions may be reduced, by writing out a few terms and bringing together both terms containing $\Delta f_n$, to

$$\Sigma\Delta f_n=0,$$

and

$$\Sigma\left(\frac{x_{n+1}+2x_n+x_{n-1}}{4}-x_0\right)\Delta f_n=0.$$

If the intervals $x_{n+1}-x_n$ and $x_n-x_{n-1}$ are equal, the last condition becomes

$$\Sigma(x_n-x_0)\,\Delta f_n=0.$$

If also $\Delta f_n$ is small, the ordinate-difference $\Delta y_n$, figure 2, between the straight line and $f_n$, the observed frequency, may be taken as $\Delta f_n/(df/dy)_n$ approximately,

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* A slight deviation has been made here from standard practice in the calculation of the moments of the Gaussian distribution, in that the quadrature is approximate.

$(df/dy)_n$ being the value in the neighbourhood of $f_n$. Moreover the equation to the straight line may be taken as

$$y = m(x - x_0),$$

and the two conditions to be fulfilled, when written in terms of $\Delta y_n$, become

$$\Sigma\,(df/dy)_n\,\Delta y_n = 0 \qquad \text{......(1)},$$

$$\Sigma\,(x_n - x_0)\,(df/dy)_n\,\Delta y_n = 0 \qquad \text{......(2)}.$$

Both these conditions are fulfilled if

$$\Sigma\,(df/dy)_n\,\Delta y_n^2 \qquad \text{......(3)}$$

is a minimum;

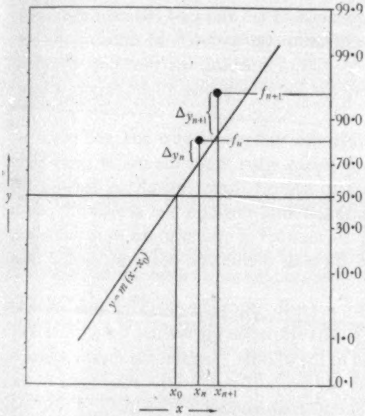

Figure 2. Relation between frequency and ordinate-differences.

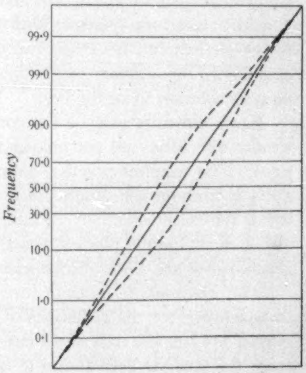

Figure 3. Importance to be attributed to deviations from the straight line at different frequencies.

for on differentiating (3) with respect to $x_0$ we find that

$$\Sigma\,2\,(df/dy)_n\,\Delta y_n\,(\partial\Delta y_n/\partial x_0) = 0,$$

or, since $\partial\Delta y_n/\partial x_0$ is equal to $-m$,

$$-2m\Sigma\,(df/dy)_n\,\Delta y_n = 0,$$

which, except for the constant term $-2m$, is identical with equation (1). Also, differentiating equation (3) with respect to $m$, we find that

$$2\Sigma\,(df/dy)_n\,\Delta y_n\,(\partial\Delta y_n/\partial m) = 0$$

or, since

$$\partial\Delta y_n/\partial m = x_n - x_0,$$

$$\Sigma\,(df/dy)_n\,(x_n - x_0)\,\Delta y_n = 0,$$

which is condition (2).

Thus the straight line representing the Gaussian distribution should be drawn through the plotted points according to the condition (3). Naturally, if this condition is to be fulfilled exactly a considerable amount of computation is involved, and the best thing then is to calculate $m$ and $x_0$ from the original data by the usual methods of statistics, and to avoid the use of probability paper entirely. But the condition (3) is still useful for indicating the weight to be placed on the individual observations when the best line is being estimated by visual judgement, for it shows that the importance to be placed on the deviation of a point from the straight line is proportional to $\sqrt{(df/dy)}$ in the neighbourhood of the point. In figure 3 this quantity is shown graphically by the ordinate separation between the two broken lines, and indicates that only half the weight should be given to deviations at frequencies of 5 and 95 per cent as compared with that given to deviations at frequencies around 50 per cent. At frequencies of 0·8 and 99·2 per cent the weight should be only one-quarter. With this difference the problem of drawing the best fitting straight line is exactly the same as that of drawing the best line, by visual estimation, for ordinary experimental results for which one might expect a least-squares solution to be the best.

Some difficulty may be felt concerning the end points at 0 and 100 per cent; similar difficulties are not unknown in statistics apart from the use of probability paper. The simplest practical solution is to neglect them entirely in drawing the straight line, on the ground that the straight line through the remaining points would remain substantially unchanged if the number of observations were increased, although the points plotted, apart from those at 0 and 100 per cent, would then reach higher and lower frequencies.

The observations already mentioned confirm the soundness of this method of weighting, for the deviations of the points from the curves are quite obviously greater towards the ends of the lines than in the middle. In fact, the mean deviation from the line has been found to be roughly proportional to the reciprocal of the above weighting factor $\sqrt{(df/dy)}$ for different frequencies.

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# THE INTEGRATION OF LARGE NUMBERS OF X-RAY CRYSTAL REFLECTIONS

By RALPH H. V. M. DAWTON, B.Sc., A.Inst.P.

*Communicated by Dr F. J. Harlow, 23 June 1938. Read in title 14 October 1938*

*ABSTRACT.* The experiments described show that if a positive film is printed from an x-ray negative, the change in the transparency of the former can be made directly proportional, over an extended range, to the intensity of the original x-ray beam. The transmitted light can therefore give the integrated x-ray value of a patch of non-uniform blackening without the necessity of a two-dimensional point-by-point integration. The application of this principle to x-ray analysis of crystal structure is discussed.

## § 1. INTRODUCTION

The advance of x-ray crystal analysis and in particular of x-ray work on proteins requires the measurement of large numbers of photographically recorded x-ray intensities and a rapid method of measuring them would be welcome. In the present paper it is shown that by making a print of the x-ray film, which will be called the negative, on another film, the positive, the change of transparency of the latter, for a considerable range of intensities, is proportional to the x-ray exposure. This enables the measurement of the film to be completed in about one-tenth of the time required by existing methods and without the use of complicated apparatus such as the alpha-ray photometer(1) or the mechanical integrator(2) at present necessary.

It must be remembered throughout the work that the usual linear {density, exposure} characteristic of x-ray films is of no help in this direct method of integrating, because the density is defined as the common logarithm of the reciprocal of the transparency. It is a linear {transparency, exposure} characteristic that is required, because optical photometers measure the light which the film transmits.

## § 2. APPARATUS AND METHOD

The photometer employed is a simple system for focusing a uniform exploring beam on the film. The transmitted light is received by a Weston Photronic cell, the current in which is directly proportional to the intensity of the incident light, so that no calibration is needed. Further, the currents are of sufficient magnitude to eliminate the need for amplification when a galvanometer with a sensitivity of about 100 mm. per $\mu$a. is used. The photocell shows a slight initial drift when exposed to light, unless the light is allowed to diverge sufficiently before striking the photocell. Some of the light which is scattered at a wide angle by the film will now fall outside the photocell but may with advantage be returned by a cylindrical mirror, such as a tube of aluminium foil. The light-source used in the experiments was a 12-volt 48-watt lamp of the projector type, but for routine work a 12-volt 100-watt lamp would be better. The lamp was operated from a floating accumulator.

The success of this method of integrating depends on the balancing of the characteristics of the negative and positive films to give a resultant linear {transparency, exposure} relation. The characteristic of the negative film was determined by the use of a cam giving a wedge of linear exposure. X-ray radiation from targets of molybdenum and of copper were used; each gave a curve of the same shape, figure 1A and 1B. In agreement with the findings of other workers(3) no induction period was observed. For our purpose the number of X-ray films from which to choose is extremely limited when compared with the number of optical films available for the positive. Further, on the positive the exposure ranges from a minimum to a maximum, and hence only a portion of the characteristic is used. This portion may be selected from the most favourable part by correct choice of the printing exposure.

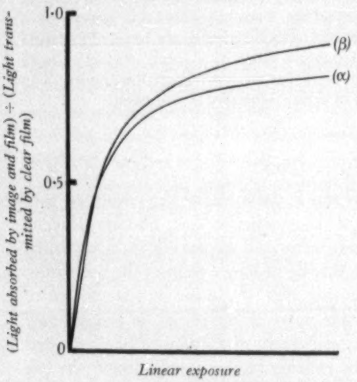

Figure 1A. Curve α: film developed by Agfa Rodinal. Curve β: film developed with the more contrasty X-ray developer.

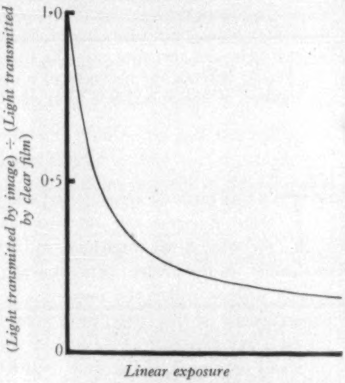

Figure 1B.

The conditions to which the positive emulsion must conform may be found as follows. Let figure 2A represent the characteristic of the negative film, the axes being as in figure 1B. Take points *A*, *B*, *C*, *D*, ... at equal intervals along the exposure axis (i.e. along the wedge); the light-intensities for printing on the positive film are then proportional to *AA′*, *BB′*, *CC′*, *DD′*, ... and we require them to produce on the positive a series of transparencies each of which differs from its neighbour by a constant amount. If we now make *OY* an exposure axis (it is parallel to the exposures *AA′*, *BB′*, ...) and *OX* a transparency axis, then *AB*, *BC*, ... become the constant differences in the neighbouring transparencies and so we obtain the requirements of the positive characteristic. This curve is redrawn in figure 2B. We note that the scale of the new axes may be chosen at will, and that the zero of the transparency axis lies anywhere on *XO* produced. It is now only necessary to select an appropriate emulsion and to choose the developer and adjust the printing exposure so as to obtain the best quality in the positive.

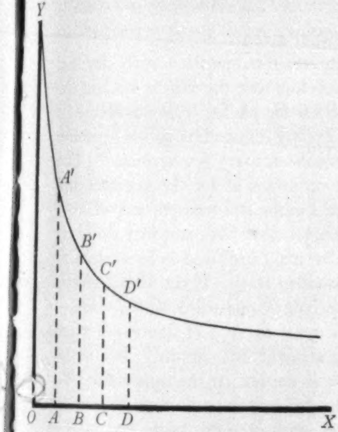

Figure 2A.

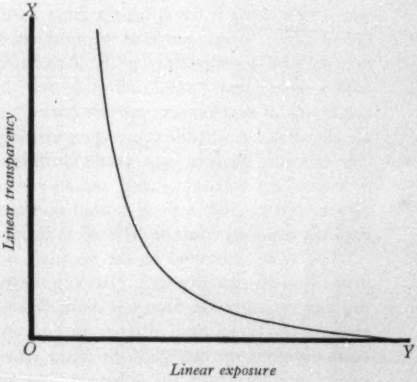

Figure 2B.

## § 3. RESULTS

*The negative film.* Two films were used, the double-coated Ilford x-ray film and the Kodak Duplitized x-ray film, both without intensifying screens. These films are first soaked in water and then developed for $6\frac{1}{4}$ min. at 16° c. with good agitation in a tank of Agfa Rodinal of strength 1 in 20. The conditions are not critical, but an underdevelopment of 25 per cent results in a loss of the feebler intensities, while an overdevelopment of 25 per cent gives a dull background. The temperature of development may be varied provided that the duration is correspondingly adjusted; for instance at 18° c. the time is reduced to $4\frac{1}{4}$ min. No experiments were made at a temperature higher than 18° c. After being fixed in an acid bath the films are washed and are dried for 24 hr.

*The positive film.* For this the Kodak Commercial film or the Ilford Commercial orthochromatic film is satisfactory; the best results were obtained by a combination of the Ilford x-ray film as the negative with the Kodak Commercial film as the positive. It is for this combination that results are given. The film is developed for

5 min. at 17° C. in the following solution, which is one of the many Ilford developers:

| | |
|---|---|
| Metol ... ... ... ... ... | 2 g. |
| Sodium sulphite (crystals) ... ... | 150 ,, |
| Hydroquinone ... ... ... | 8 ,, |
| Sodium carbonate (crystals) ... | 100 ,, |
| Potassium bromide ... ... ... | 5 ,, |
| Distilled water, up to ... ... | 1 l. |

The components are to be dissolved in the order given.

It is necessary, before development, to saturate the emulsion with distilled water by soaking it for at least 2 min. After development the film is washed for about $\frac{1}{2}$ min., fixed, and after being washed, dried for 24 hr. The possibility of non-uniform development was found to be very greatly reduced by gentle brushing with a rubber strip moved uniformly over the emulsion every few seconds(4). The conditions of development are not critical, and variations as for the negative film are allowable. A suitable printing exposure gives a minimum transparency of 0·01. The exposure tends to vary a little for different batches of film, and may do so by ± 20 per cent without causing serious error. The exposure used is equivalent to $2\frac{1}{2}$ sec. at 6 ft. from a small frosted 15-watt gas-filled lamp. If the Ilford film is used the exposure must be reduced to about one-half or one-third of this value.

The x-ray intensities on the negative, which gave curve $\alpha$ of figure 1A when printed on the positive film, give very nearly a straight line, figure 3; but if the printing exposure has been too short, this curve is convex on the upper side. To measure the actual ratio of intensities we use on the positive a uniform exploring beam of light large enough to cover the reflection, and place this beam first by the side of, and then over, the reflection. The difference in the light transmitted gives at once a measure of the x-ray intensity. If the reflection is so close to another that there is insufficient space to allow the exploring beam to be directed on to the background, the beam may be temporarily reduced in size and the observed background may be correspondingly increased; this is necessary because owing to scattering from unwanted sources, such as air, the background over the film is not constant. A test was made on an x-ray negative containing several reflections already measured on the mechanical integrator. A print of this film was made and the reflections were measured directly from the positive. The results are given in the table. The reflections at the end of the table are the weak reflections. For all such small exposures the characteristic of the negative film is linear and steep, and therefore, provided the background also is small, the reflections may be measured directly from the negative film; results obtained in this way are given in column 4. Measurements so made on the negative film are far more satisfactory than results evaluated by the mechanical integrator. In practice the best rapid method of measuring a group of intensities is to measure the weak reflections directly on the negative film and the remainder on the positive film, the same photometer serving for both sets of measurements. The correlating factor between them can be accurately found from a knowledge of (1) the slope of the characteristic, figure 1A, of the negative film in the region of weak intensities, (2) the slope of the linear {transmission, exposure} graph, figure 3, of the positive film, and (3) the relative intensities of the exploring points used in the photometering of the negative and positive films. Full details are not given in this paper.

It will be noted that the differences between column 2 and column 3 of the table are considerably greater than the errors in a previous paper(2), in which the ratio of the reflecting power of X-ray planes is given. This is because the intensities of these X-ray planes were each an average of a group of individual reflections, whereas in the present paper the individual measurements themselves are given.

| Identification number of X-ray reflection | Integrated value by the mechanical integrator | Value by direct integration from positive film | Value by direct integration from negative film, valid only for weak reflections |
|---|---|---|---|
| 1 | 6·5 ± 5 per cent | 6·8 | — |
| 2 | 24·5 ± 4 ,, | 24·9 | — |
| 3 | 13·7 ± 4 ,, | 12·4 | — |
| 4 | 22·2 ± 7 ,, | 23·2 | — |
| 5 | 37·6 ± 3 ,, | 36·0 | — |
| 6 | 10·9 ± 9 ,, | 10·4 | — |
| 7 | 7·6 ± 3 ,, | 6·8 | — |
| 8 | 13·0 ± 5 ,, | 12·2 | — |
| 9 | 22·4 ± 3 ,, | 23·5 | — |
| 10 | 22·8 ± 3 ,, | 21·8 | — |
| 11 | 53·7 ± 5 ,, | 53·0 | — |
| 12 | 29·7 ± 3 ,, | 30·5 | — |
| 13 | 6·2 ± 5 ,, | 6·0 | — |
| 14 | 22·2 ± 5 ,, | 26·0 | — |
| 15 | 27·7 ± 4 ,, | 27·6 | — |
| 16 | 64·1 ± 5 ,, | 63·5 | — |
| 17 | 55·1 ± 4 ,, | 55·0 | — |
| 18 | 20·4 ± 4 ,, | 20·0 | — |
| 19 | 1·8 ± 12 ,, | 2·1 | 1·8 |
| 20 | 3·9 ± 12 ,, | 3·4 | 3·9 |
| 21 | 3·4 ± 8 ,, | 2·7 | 3·5 |
| 22 | 4·1 ± 8 ,, | 3·9 | 4·2 |
| 23 | 4·3 ± 5 ,, | 4·8 | 3·7 |
| 24 | 4·7 ± 5 ,, | 3·7 | 4·2 |

## § 4. LIMITATIONS OF THE METHOD

It is to be observed in figure 3 that a departure from linearity occurs in the region of greatest X-ray exposure, and this circumstance limits the range of measurable intensities. The restriction is not entirely new, because the flattening of the negative characteristic has, in the past, limited the range of accurately measurable intensities. The full range is usually covered by correlating a few photographs of differing exposures or by the artificial reduction of the stronger intensities by shutters incorporated in the X-ray camera(5).

Fundamentally, a mechanical integrator affords the most accurate method of evaluating photographically recorded intensities, since it puts reliance on one film

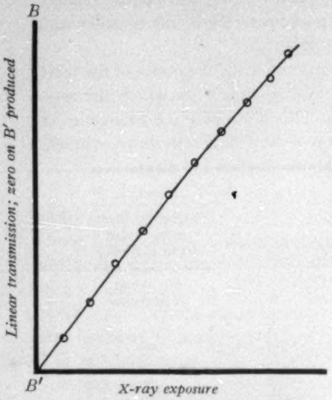

Figure 3. $B'$ represents the light transmitted through positive at the foot of the (printed) wedge.

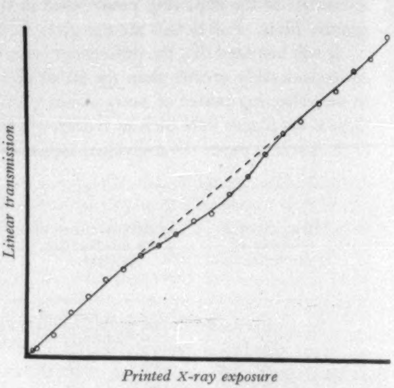

Figure 4.

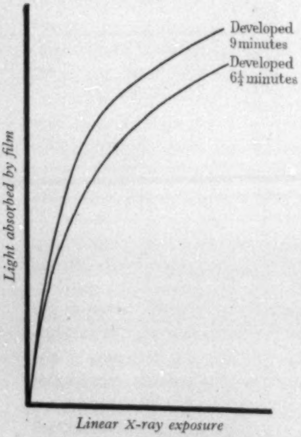

Figure 5A. Negative film.

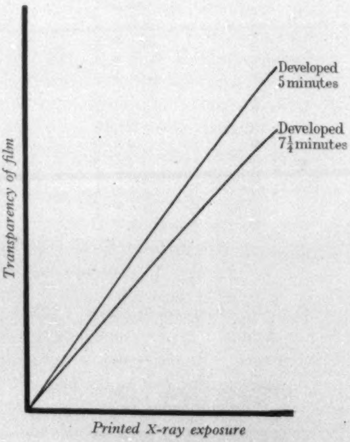

Figure 5B. Positive film.

only, i.e. the negative. It must, however, be observed that full reliance is put on the master wedge which not infrequently is found to contain faults. Such faults are often not apparent until a positive print of the wedge is inspected; a distortion occurring in the resulting straight line is more easily observed than one occurring in the curved characteristic graph of the negative. An example of such an error shown by the positive is given in figure 4.

High precision in the measurement of individual X-ray reflections is seldom obtained and, although desirable, is in fact not absolutely necessary for all structure analysis work. Further, the infinite Fourier series itself requires some compromise between accuracy and convenience. There seems no doubt that in X-ray work integration by the positive film can serve a useful purpose by the rapidity of measurement and the simplicity of apparatus with which it is associated. The main disadvantage is that it involves the use of a second film with its attendant errors, one error being due to increase of development towards the edge of the film. This occurs also in the negative film and it is of interest to observe that in summation the two errors tend to mutual cancellation; overdevelopment of the negative increases the apparent X-ray intensities, figure 5A, and in the positive decreases them, figure 5B.

## § 5. ACKNOWLEDGEMENTS

In connexion with the latter part of this work the author is indebted to Dr J. Monteath Robertson, who had himself begun work on similar lines, for criticism and suggestions concerning the results, and for the loan of the X-ray negative containing the reflections given in table 1. The author also thanks Dr L. Lownds of Chelsea Polytechnic for his interest in the work, and Messrs Kodak, Ltd., for samples of film.

### *Note added in proof*, 2 *October* 1938

Results for the Ilford Ilfex film also have been obtained. As a positive film the Kodak Commercial orthochromatic film was used and was treated in the same way as the Kodak Commercial film in this paper, except that the printed exposure was considerably reduced, being about 2 sec. at 7 ft. from a 5-watt frosted vacuum lamp, and giving a minimum transparency of about 0·04.

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# A CONCAVE-GRATING VACUUM SPECTROGRAPH FOR WAVE-LENGTHS 15 TO 1000 A.

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*Read in title* 14 *October* 1938

*ABSTRACT.* A vacuum spectrograph for work with soft X rays is described. It employs a 1-metre concave grating in the grazing-incidence position. The instrument is designed to permit the optical system to be adjusted before it is placed in the vacuum chamber, so that trial-and-error focusing may be minimized. It makes provision for varying the grazing angle without any necessity for readjustment. Tests of the instrument are described and discussed.

## § 1. INTRODUCTION

A CONCAVE grating in the grazing-incidence position was first used for the study of soft X rays by Osgood(1) and for the vacuum-spark spectra by Hoag(2). Later some very fine spectrograms from the vacuum spark were obtained by Ericson and Edlén(3) who used an instrument of Siegbahn's design, and more recently a number of publications(4) have described vacuum spectrographs in which this arrangement has been devoted to one or other of these types of spectra. In the following paper we describe an apparatus of this kind which embodies some new features.

In designing the apparatus several requirements have been specially kept in mind. First, it was our object to build an instrument with good vacuum properties, and having as few welds and seals as possible. Secondly, it was desirable that gases from the spectrograph should not readily diffuse into the X-ray tube; this requires that the narrow slit of the optical system shall be the only opening for such diffusion. Thirdly, it was proposed to evolve an apparatus which would allow the adjustments of the optical system to be made in such a way as to minimize trial-and-error focusing. The apparatus was to cover the range 15 to 1000 A. and to allow the use of different angles of incidence without readjustment. To permit of good grating reflection at the shortest wave-lengths, provision was required for a small minimum grazing angle, actually rather less than 2½°. The main purpose of the apparatus was the study of the soft X rays from solids. For this, high dispersion and resolution are of less importance than good intensity and ease of working. A short-focus instrument is therefore suitable, and the slits need not be inconveniently narrow. Finally, precautions were necessary to prevent the plates from being fogged by visible light entering the spectrograph through the slit.

## § 2. APPARATUS

*The vacuum chamber*. The vacuum chamber is a steel cylinder with a single longitudinal weld, and with wide flanges welded to the ends to receive end plates, which are of rolled brass. Each plate is ground to its flange, and in use the joint is made air-tight by the application of Apiezon sealing compound $Q$, which is smeared on externally. To facilitate the sealing process the plates are initially clipped tightly in position by the devices $A_1$ and $A_2$, figures 1 and 3. The end plates have bevelled edges so as to receive the sealing compound readily. This method of sealing has been found both speedy and reliable, but experience has shown that more clearance between the outside edges of the end plates and the clips would be advantageous.

*The slit-mounting*. One of the end plates is pierced by a circular hole in which the slit-holder fits and in which it can rotate. The rotation is controlled by means of the screws $S_1$, figure 4. The slit-blades are of stainless steel and are screwed to the holder, which is machined so that the blades lie in a plane approximately normal to the radiation. As the slit is only 1 cm. long and of the order of 0·01 mm. wide, there is little diffusion through it at low pressures. The flange on the slit-holder obviates any other leakage to the X-ray tube.

*Optical bench*. A machined brass bracket $B$, figures 1 and 4, is rigidly attached to the end plate which carries the slit. The runway carrying the grating carriage and camera is pivoted in this bracket at $P$, figure 3. It has on it a guide rail $R$, figures 3 and 4, machined to the radius of curvature of the Rowland circle. The grating carriage and camera are held closely in register with the rail by spring-loaded plungers. In making this register surface in the lathe, a false piece was fastened to the face plate on the opposite end of the diameter and turned with it to the correct diameter as measured by means of a gauge set to the measured radius of curvature of the grating. The camera and the bearing surface of the grating carriage are similarly turned to the required curvature.

*Grating and grating-holder*, figure 5. A Siegbahn glass grating having 576 lines per mm., rulings 1 cm. long, ruled portion extending over 2 cm., and radius of curvature 99·54 cm. is employed.* The movement of the grating backwards and forwards is achieved by means of the milled nut $N$, figure 4, acting against a spring pressing against a shoulder of the spindle $Q$. To avoid twist, the spindle is hexagonal in section. A ball joint $J$, figure 1, is provided to permit the rotation of the grating in its own plane and to allow it to be tilted backwards and forwards; the lever $L$, figures 1 and 4, imparts these motions. Finally, so that the grating may turn about a vertical axis, the whole system is mounted on a small table $T$, figure 4, which has a conical bearing. The axis of rotation passes vertically through the register surface and through the centre of the surface of the grating. A rotation should not, therefore, move the grating off its position over the Rowland circle rail. Adjustment of the tilt will, however, have a small effect on this position.

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* In the usual mounting and with a grazing angle of 2·5°, this yields a dispersion of 2 A. per mm. at 100 A., and 2·7 A. per mm. at 200 A.

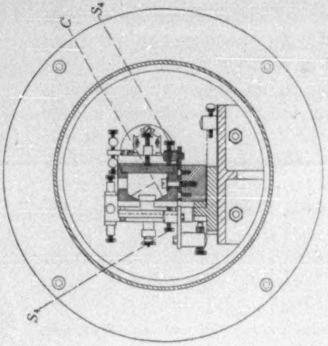

Figure 2. Section at 1, in direction of arrow.

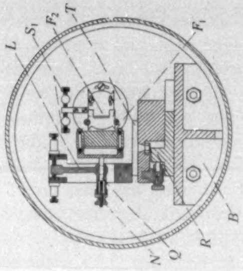

Figure 4. Section at 2, in direction of arrow.

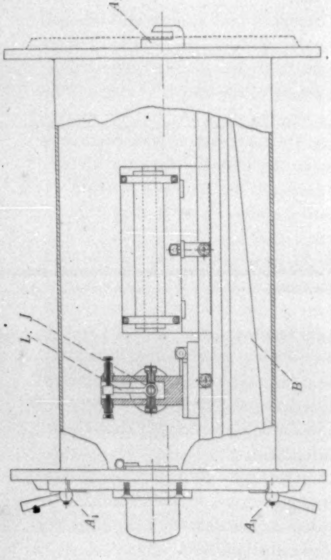

Figure 1.

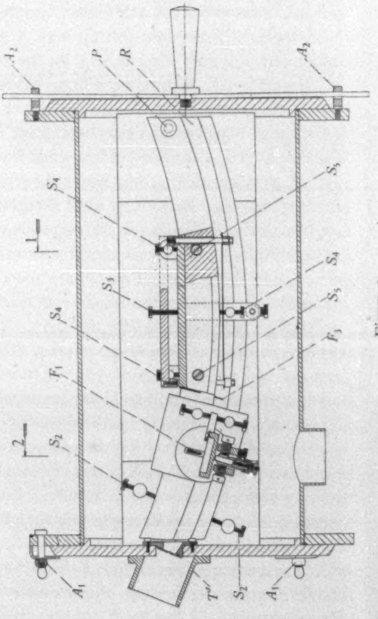

Figure 3.

*The camera*, figure 6. The camera is made in the form of a box open at the two ends—at one end to permit the entry of the radiation, at the other to let stray light pass out. After the box had been made, the side to receive the photographic plate was turned to the radius of the Rowland circle. The internal surfaces *C*, figure 2, are bevelled in order that no light reflected from them may strike the plate.

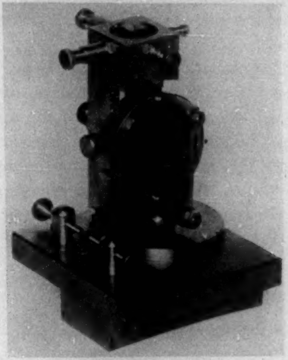

Figure 5.

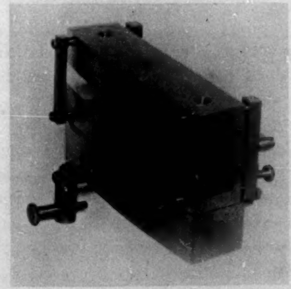

Figure 6.

To prevent the plate from being fogged from the back it is always mounted with a flexible sheet of brass behind it, and to minimize fogging due to scattering in the box the metal surfaces are coated with Aquadag.* The photographic plate is pressed to the curved surface of the camera by means of a clamp with a single central screw $S_3$, figure 3, which actuates rods passing through the ends of the camera. The camera box is machined on its lower surface and can slide in a radial direction on the upper surface of its carriage. Trial-and-error focusing adjustments can therefore be made by means of the screws $S_4$, figures 2 and 3. The camera can be rigidly fixed to its carriage by the screws $S_5$, figure 3. To permit these to be tightened, two holes are drilled in the top of the camera. The screen $F_1$, figures 3 and 4, opposite the face of the grating, the sliding screens $F_2$, figure 4, close to the slit, and $F_3$, figure 3, at the entrance to the camera, are all designed to cut down the fogging of the plate.

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* Kindly supplied by Messrs E. G. Acheson, Ltd.

*Adjustment.* The following adjustments are made before the apparatus is placed in the steel chamber.

(*a*) To locate the slit on the Rowland circle, the jig, figure 7, has been constructed. Its surfaces have been turned to the curvature of the Rowland circle. A slit-blade is removed and the jig is clamped to the rail with its point passing through the slit-aperture. The slit-holder is then rotated and the runway adjusted by screws $S_2$, figure 3, until the surface of the jig makes contact with the slit-blade. The jig is then withdrawn and, with the aid of a microscope, the second blade is placed parallel to and at the required distance from the first.

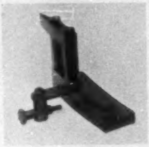

Figure 7.

(*b*) To set the grating with its pole on the prescribed circle, an optical bench is placed immediately below the base bracket and so that the curved rail crosses it approximately at right angles. A pointer is mounted on a carriage on the bench. The jig is then placed on the circular track, and the pointer carriage is moved until the tip of the pointer touches the curved surface of the jig. The jig having been withdrawn, a microscope is mounted on a stand on the optical bench and slid along until it focuses on the end of the pointer. Finally, the grating carriage is placed on the track, moved until its centre is opposite the microscope, and then adjusted until the microscope focuses on the pole of the grating. Actually we focused on particles of dust on the surface.

(*c*) The next step is to make the grating, at its pole, tangential to the prescribed circle. First, the camera is adjusted so that its cylindrical surface is continuous with that of the camera carriage. This is done with the aid of a straight edge. Next, the camera is placed on the circular track at the end farthest from the slit. A fine hair is fixed vertically between the bevelled edges of the camera, and therefore on the circle, again at the end farthest from the slit. The grating and camera are now moved until slit and hair are equidistant from the pole of the grating. A distance piece is used for this adjustment. The grating is then rotated about a vertical axis until the image of the slit coincides with the hair. A microscope is employed to observe this coincidence.

(*d*) The grating must be tilted backwards or forwards until the image of the slit is as sharp as possible. This test is best made photographically.

(*e*) The final adjustment consists in the rotation of the grating in its own plane in order that the rulings may be made parallel to the slit. It is convenient to place a sodium lamp in front of the slit and to observe the $D$ lines with an eyepiece. The grating is adjusted to make them as sharp as possible. This adjustment is not very critical.

In case the later settings may have affected them, the settings (*b*) and (*c*) should be checked and any necessary small readjustment should be made. The knife-edge screen $F_1$, figures 3 and 4, is then placed in position. Care should be taken that this process shall not disturb the grating. The Rowland circle track is now placed in the vacuum chamber, the end plate, on which it is permanently mounted, being provided with two detachable handles to assist in this process. The metal tube $T''$, figure 3, is next screwed into place in front of the slit, and the X-ray tube fitted over it. The joints are waxed. In the experiments carried out so far, a quartz X-ray tube has been employed, figure 8. The transparency of the quartz is convenient for setting the electrodes, but on the other hand a quartz tube is, of course, not so easily cooled as a metal one. The water cooling of the focusing cylinder around the filament was found to be necessary to check the deposition of carbon on the anticathode. For work with the vacuum spark, the anticathode and filament are replaced by conical water-cooled electrodes.

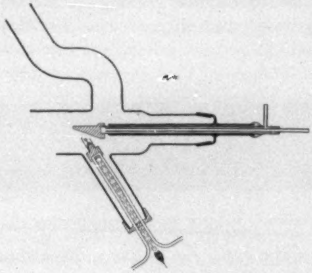

Figure 8.

The X-ray tube and spectrograph are evacuated through independent wide tubes by a Metropolitan-Vickers oil diffusion outfit, the tubes uniting at the mouth of the pump. Large metal stopcocks are placed in each of the tubes so that the air can be let into the spectrograph at the end of an exposure without entering the pumps and oxidizing the hot oil. A connecting tube with stopcock allows the pressures in the spectrograph and X-ray tube to be equalized when air is being admitted to the spectrograph. Tombac bellows are included in the pumping lines to check vibration from the backing pump, and brackets are mounted on the spectrograph to prevent stresses on the X-ray tube when the bellows collapse. A condensation trap is mounted in the X-ray tube line as a precaution. The bends in the X-ray tube outlet, figure 8, are to prevent electrons from reaching the seal of the pumping line.

## § 3. RESULTS

The vacuum properties of the spectrograph are very satisfactory. The breaking and making of the seal of the end plate for the purpose of loading the camera can be speedily carried out.

It was necessary to have photographic films or plates which could be bent to a radius of curvature of 50 cm. The thinnest glass plates that we could procure broke under such severe bending. Experience showed that films always buckled in the camera after the spectrograph had been pumped out. This, of course, rendered them useless for grazing-incidence work. Some mica sheets were then coated with emulsion for us by Messrs Ilford, Ltd.* The emulsion, however, tended to peel off under the vacuum conditions. Finally we succeeded in obtaining glass blanks of thickness 0·4 mm.,† and these, coated by Messrs Ilford, Ltd., have proved satisfactory.

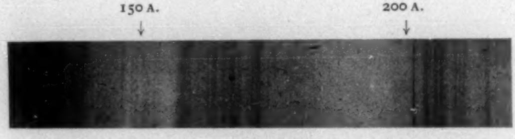

Figure 9. Copper spark. Magnification 3·6.

For soft X-ray work in the region up to 100 A., we had previously found Ilford Process plates very satisfactory. For vacuum-spark spectra we have employed Ilford Q 2 emulsion. This not only is satisfactory in the region above 100 A., but also was found to be much more sensitive than the Process emulsion even for wavelengths as short as 14·5 A. (nickel $L_\alpha$). The plates are easy to handle and do not show chemical fog.

The precautions against optical fogging proved satisfactory. Of the screens *F*, that immediately at the end of the photographic plate proved essential. Without it, light entered the end of the glass plate, and, suffering multiple reflections, produced a periodic blackening along the emulsion. Reflection from the back of the grating was diminished by coating the latter with a mixture of lampblack and Canada balsam.

The apparatus was set up in the manner described above, and a number of exposures were taken with soft X rays down to 14·5 A. and with the vacuum spark up to 800 A. Adjustments (*a*), (*b*) and (*c*) were then remade and more exposures were made. In neither case was trial-and-error focusing employed. Figure 9 shows a copper vacuum-spark spectrogram taken with grazing angle $2\frac{1}{2}°$ and slit-width 0·009 mm. The theoretical resolving power under these conditions is limited by the slit-width, and may be calculated from the formula

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* Through the courtesy of Dr O. Bloch.

† Procured from Messrs James Hetley and Co., 35 Soho Square, W. 1.

$$R=\frac{0{\cdot}91\,\rho\lambda m}{s\sigma},$$

given by Mack, Stehn and Edlén$^{(5)}$, where $\rho$ is the radius of curvature of the grating, $s$ is the slit-width, $\sigma$ the grating constant, and $m$ the order. This yields the value 1160 at 200 A. The observed value is not very different from this, and justifies our focusing system. For higher resolution the slit would need to be narrower, and then possibly some trial-and-error focusing might become necessary for the best results. On the other hand, in order to obtain good reflection for the shortest wave-lengths, we have used small grazing angles. With larger angles focusing errors would be considerably diminished. Again, we have here increased the risk of focusing error by employing a grating-width greater than the optimum.*

For soft X rays of the shortest wave-length, the diffracted beam makes a very small angle with the grating and with the photographic plate. Small errors in position of the surface of the plate consequently become of greater importance; it is therefore not surprising that the spectra are not consistently satisfactory below about 50 A. However, with the grating which we employ, reasonably good intensity can be obtained in the fourth order, and we are able to examine the short-wave spectra without working in the more difficult portion of the Rowland circle. If the early orders of the short-wave spectra are to be examined, it would appear preferable to employ the Söderman method$^{(4)}$, in which the plate is mounted perpendicular to the radiation.

## § 4. ACKNOWLEDGEMENTS

The spectrograph was built by Mr T. Gurman in the workshops of the Mechanical Engineering Department of this College. The apparatus has been erected and tested in the Physics Department. We have pleasure in expressing our gratitude to the respective heads of these Departments, Prof. G. T. R. Hill and Prof. E. N. da C. Andrade. In addition, we are indebted to various members of the Engineering Faculty for valuable discussions.

Finally we are indebted to the Dixon Fund of the University of London for a grant to one of us (F. C. C.) which made the construction of the apparatus possible.

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(5) Mack, Stehn and Edlén. *J. Opt. Soc. Amer.* **22**, 245 (1932).

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* The restricted size of the spark gap, however, probably diminished the effective size of the grating slightly. The width for maximum resolving power has been calculated by Mack, Stehn and Edlén$^{(5)}$.

# THE ACCURACY OF RECTIFIER-PHOTOELECTRIC CELLS

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*ABSTRACT.* The effect of a resistance $R$ in series with a rectifier-photoelectric cell on (*a*) the linearity of the response and (*b*) the temperature coefficient of the sensitivity has been investigated. In order that no restriction should be imposed on $R$ by the effective sensitivity, the photoelectric current was measured by compensation. The light sources used in these experiments were tungsten filament vacuum lamps operating at a colour temperature of approximately 2400° K. The temperature coefficient of the cell is always least when $R=0$, but is not then generally zero. The approach to linearity is generally closest for a finite value of $R$ which is usually between 100 and 1000 $\Omega$. But no value of $R$ will avoid errors of 1 per cent if linearity is assumed over a range of 0 to 240 lux. The observations can be explained qualitatively, but not completely, by assuming the presence of a series resistance inside the cell in addition to the shunt resistance. These two resistances do not vary in the same manner with temperature or with illumination. Some practical conclusions are given in the final paragraph.

## § 1. THE PROBLEM

The output of a rectifier cell is usually measured by connecting it in series with a resistor of resistance $R$, as shown in figure 1 but without the dotted element, and measuring the current $I$ through the resistor. The resistor may be a galvanometer or microammeter; but if high accuracy is required, it will generally be a standard resistor and the voltage $V$ across it will be measured with a potentiometer. The relation between $I$ and $L$, the luminous flux incident on the cell, can then be represented by

$$I=\alpha L\{1-f(L,R)\} \qquad \text{......(1)},$$

where $f=0$ when $L=0$.

The earlier investigators, aiming at an accuracy not greater than 1 per cent, concluded that $f$ was always positive and increased both with $L$ and $R$. This conclusion was explained by taking into account the shunt resistance $r$ of the cell, which is known to decrease as $L$ increases; the circuit should include the dotted element, the cell $C$ being regarded as a resistanceless source of current $I_0$, proportional to $L$. Then

$$f(L,R)=R/(R+r) \qquad \text{......(2)}.$$

It followed that, if $R$ could be made zero, $f$ would always be zero and the cell would be truly linear.

More recent experiments aiming at a higher accuracy, and in particular the very careful work of Buchmüller and König(1), throw doubt on this conclusion. They seem to show that $f$ may sometimes be negative and that, even if it is positive, it does not necessarily tend to zero with $R$. It may be said at once that this is the result to which our experiments lead. But previous work has been hampered by the impossibility of reducing $R$ without limit while retaining adequate sensitivity in the measurement of the current; for, whatever is the nature of $R$ in figure 1, the sensitivity inevitably decreases with $R$.

Restrictions on the value of $R$ can be removed by using the compensation method of measuring $I$, proposed by Campbell and Freeth(2). Their circuit is shown in figure 2, but the resistance $R$ was not mentioned originally. The rheostat $Q$ is varied until the galvanometer $G$ is undeflected, showing that there is no potential difference between the points $X$, $X$; then the current flowing through the direct-reading instrument $A$ must be the same as that flowing through $R$. The sensitivity is determined wholly by that of $A$, which may have any resistance; $R$ does not affect the current sensitivity and may be zero. A simple modification of figure 2, more convenient in practice, is shown in figure 3. Here $P$ is an accurate potentiometer, such as might be used for measuring $V$ in figure 1 but with a higher range. If $V$ is read on the potentiometer in the ordinary way, i.e. as proportional to $p/P$, where $P$ is the total resistance of the potentiometer, the maximum relative error involved in taking $I$ to be proportional to $V$ is $P/4S$, where $S$ is the resistance through which the compensating current passes. If, for example, $S = 30{,}000\ \Omega.$, $P = 30\ \Omega.$, the maximum error is 1 part in 4000. It is to be observed that the sensitivity, taken as the value of $V$ corresponding to a given $I$, is increased in the ratio of $S$ to $R$ as compared with that pertaining to figure 1. In practice this means that, while $V$ is in millivolts in figure 1, it is in volts in figure 3.

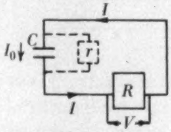

Figure 1.

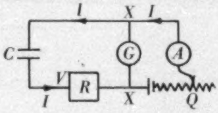

Figure 2.

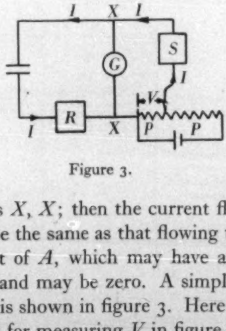

Figure 3.

This method has been used to determine the departure from linearity, in dependence on the value of $R$, of several rectifier cells of different types; that is to say, the quantity $f(L, R)$ has been determined; the dependence on $R$ of the temperature coefficient of $I$ has been observed also. $L$ was varied, as it must be in any experiments aiming at accuracy, by addition. The principle is well known. The cell is arranged so that it can be exposed to any combination of $N$ independent sources of light. The sources are adjusted so that each of them, acting apart from the others, produces the same current in the cell; each source then throws the same amount of light $L_m$ on the cell. The light received by the cell when $n$ sources act together is $nL_m$.

However, as many workers have found, it is not easy to apply the principle exactly. It is necessary to ensure that the exposure of the cell to light from one source shall not change the light received from another; it is necessary also to keep the temperature of the cells constant. Accordingly we give details of our apparatus and of the conduct of the observations.

## § 2. APPARATUS

The apparatus is shown diagrammatically in figures 4 and 5. It was constructed in 1933,* and was originally used for investigations into the linearity of response of emission-type vacuum photocells. In the work on these cells the stability of the apparatus had been shown to be very high, and repetition accuracies of the order of a few parts in ten thousand had been recorded; vacuum cells had been proved linear to one part in a thousand; the suitability of the apparatus for the purpose in view had therefore been already established.

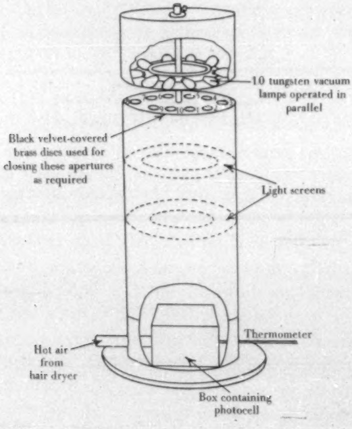
10 tungsten vacuum lamps operated in parallel
Black velvet-covered brass discs used for closing these apertures as required
Light screens
Thermometer
Hot air from hair dryer
Box containing photocell

Figure 4. General arrangement of apparatus.

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* It was demonstrated at the Physical Society's Exhibition in January, 1934.

Ten straight-wire cage-filament tungsten vacuum lamps, mounted axially and rigidly clamped, form the light sources. These are connected in parallel by leads soldered to the lamp terminals, in order to avoid the possibility of instability due to poor contacts. The lamps are operated from a battery and kept at a constant voltage throughout the tests by means of a potentiometer. In the present investigation, the operating temperature of the filaments was approximately 2400° K. Below each lamp is an aperture which can be closed, as required, by means of a brass disc covered with black velvet. The location of the apertures, lamps and photometric screens is such that the casings of the lamps and of the photocells can be kept in a fixed position, and yet there is sufficient room to enable the discs for covering the holes to be manipulated without any risk of stray light reaching the photocell.

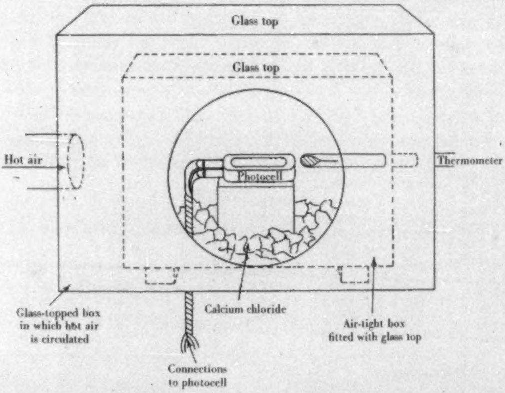

Figure 5. Sketch showing details of double box containing photocell.

The photocell under test is housed in a small box with a glass top, as shown diagrammatically in figure 5. A small quantity of calcium chloride is put in the bottom of the box in order to prevent condensation on the glass window, and a thermometer is arranged with its bulb adjacent to the photocell. This is enclosed in an outer container, and air is circulated between the walls of the two boxes by means of an electric hair-dryer, the temperature of the air being controlled by regulating the current through the heater coils of the dryer.

Precision potentiometers were used for measuring the potential drops in the photocell circuits.

## § 3. TESTS ON APPARATUS

Before the investigations were begun tests were again made to determine the reliability of the photometric arrangement. It was found that the error introduced by light reflected from the underside of the velvet-covered discs could not be detected, and was therefore negligible. Tests on the photometric screening also showed that no measurable error could arise from this cause.

When measurements of the resistance of the photocells were being made for this investigation, reversal measurements indicated that the cells did not rectify appreciably under these conditions. With the various circuits used, no contact e.m.f. could be detected which would affect the validity of the measurements.

The double container for the photocell under test was very satisfactory from the point of view of thermal stability, and no difficulty was experienced in maintaining constant air temperatures in the inner container to within ±0·5° C., throughout any one series of measurements.

## § 4. METHOD

Table 1 shows a typical series of observations.

Table 1. Cell No. 8; $R=0$

| $n$ | $I_n$ (μa.) | $J_{10}$ (μa.) | $a_n$ | $A_n$ | $J_n$ (μa.) | $J_{10}$ (μa.) | $B_n$ | $\frac{A_n-B_n}{A_n}$ (per cent) | $\frac{\Sigma\delta_n L}{nL_m}$ (per cent) |
|---|---|---|---|---|---|---|---|---|---|
| | | 13·950 | | | | — | | | |
| 1 | 1·388 | | 0·0994 | 0·0994 | — | | 0·0994 | 0 | +4·4 |
| | | 13·948 | | | | 13·807 | | | |
| 2 | 1·318 | | 0·0945 | 0·1939 | 2·710 | | 0·1963 | $-1{\cdot}2_4$ | +1·8 |
| | | 13·938 | | | | 13·800 | | | |
| 3 | 1·348 | | 0·0967 | 0·2906 | 4·120 | | 0·2987 | $-2{\cdot}7_9$ | +1·8 |
| | | 13·900 | | | | 13·793 | | | |
| 4 | 1·233 | | 0·0913 | 0·3819 | 5·440 | | 0·3944 | $-3{\cdot}2_8$ | +0·3 |
| | | 13·883 | | | | 13·783 | | | |
| 5 | 1·443 | | 0·1040 | 0·4859 | 6·943 | | 0·5048 | $-3{\cdot}9_0$ | +2·1 |
| | | 13·868 | | | | 13·738 | | | |
| 6 | 1·213 | | 0·0875 | 0·5734 | 8·213 | | 0·5982 | $-4{\cdot}3_9$ | +0·4 |
| | | 13·850 | | | | 13·728 | | | |
| 7 | 1·448 | | 0·1046 | 0·6780 | 9·713 | | 0·7083 | $-4{\cdot}4_3$ | +1·3 |
| | | 13·833 | | | | 13·708 | | | |
| 8 | 1·388 | | 0·1002 | 0·7782 | 11·117 | | 0·8120 | $-4{\cdot}3_5$ | +2·2 |
| | | 13·820 | | | | 13·680 | | | |
| 9 | 1·093 | | 0·0792 | 0·8574 | 12·268 | | 0·8976 | $-4{\cdot}6_9$ | +0·1 |
| | | 13·807 | | | | 13·660 | | | |
| 10 | 1·303 | | 0·0945 | 0·9519 | — | | 1·0000 | $-5{\cdot}0_6$ | 0 |
| | | 13·807 | | | | | | | |

Mean 0·09519

$$\sum_1^n A_n = 5{\cdot}2906; \quad \sum_1^n B_n = 5{\cdot}5097; \quad 1-k=\frac{\Sigma B_n}{\Sigma A_n}=1{\cdot}041_5.$$

Here $I_n$ is the current with the $n$th aperture alone open, $J_n$ is the current with all the apertures 1 to $n$ open, $a_n = I_n/J_{10}$, $A_n = \sum_1^n a_n$ and $B_n = J_n/J_{10}$.

$I_n$ is measured first in the order 1 to $n$, each measurement being taken between a pair of measurements of $J_{10}$; $J_n$ is then measured similarly. The whole series of measurements takes about 15 min. Of course $B_1 \equiv A_1$ and $B_{10} \equiv 1$. The differences in $J_{10}$ indicate the repetition accuracy of a single absolute measurement. It may be questioned whether relative accuracy, which alone matters, is increased by taking $a_n$ or $B_n$ instead of $I_n$ or $J_n$. The answer is that a review of the whole series of measurements shows that regularity and consistency are thereby increased, but it is impossible to say what residual inaccuracy due to the variations of $J_{10}$ remains. The variations are probably due partly to variations in temperature of the cell, independent of the incident light; the effect of this part must be eliminated almost completely. But they are probably partly due to the effect of light on the cell in changing its temperature or causing fatigue. The effect of this part will not be completely eliminated; for the light incident when $I_n$ or $J_n$ is measured is always less than when $J_{10}$ is measured. It is thought, however, that no $a_n$ or $B_n$ is in error by more than 2 parts in 1000, and that the average error does not exceed 1 part in 1000.

Some of the measurements were repeated by the method of figure 1, with the same value of $R$. The values of $A_n$ and $B_n$ should be precisely the same, whichever method is used; for $I$ should be the same whether the p.d. across $XX$ is made zero by connecting these points by a conductor of zero resistance, figure 1, or by introducing a compensating current so as to make the deflection of $G$ zero, figure 3. Some puzzling discrepancies were found; but they could not be repeated, and it was concluded that they arose wholly from the greater experimental error of figure 1, which was due to the much lower sensitivity. No consistent difference could be found between the mean sensitivity of the cell in amp./lumen and its variation with temperature as measured by one method and the same quantity as measured by the other.

We have now to ask what information is given by these measurements. Let $L_n$ be the illumination on the cell through the $n$th aperture. All the $L_n$'s are not exactly equal; let $L_m$ be the mean of the $L_n$'s, so that

$$L_n = L_m + \delta_n L.$$

Let $L_n' = \sum_1^n L_n$ be the illumination through all the apertures 1 to $n$. Since $R$ is constant through a series, we can write $f(L)$ for $f(L, R)$. Then we have, omitting terms in powers of $f$ higher than the first,

$$I_n = \alpha L_n \,[1 - f(L_n)] \qquad \text{......(3)},$$

$$J_n = \alpha L_n' \,[1 - f(L_n')] \qquad \text{......(4)},$$

$$a_n = (L_n/L_{10}')\,[1 - f(L_n) + f(L_{10}')] \qquad \text{......(5)},$$

$$A_n = (1/L_{10}')\,[L_n' \{1 + f(L_{10}')\} - \Sigma L_n f(L_n)] \qquad \text{......(6)},$$

$$B_n = (1/L_{10}')\,L_n' \{1 - f(L_n') + f(L_{10}')\} \qquad \text{......(7)},$$

$$\frac{A_n - B_n}{A_n} = \frac{L_n' f(L_n') - \Sigma L_n f(L_n)}{L_n' \{1 + f(L_{10}')\} - \Sigma L_n f(L_n)} \qquad \text{......(8)},$$

$$= f(L_n') - \frac{\Sigma L_n f(L_n)}{L_n'} \qquad \text{......(9)}.$$

If all the $\delta_n L$'s were zero, we could write in place of equation (9)

$$\frac{A_n - B_n}{A_n} = f(nL_m) - f(L_m) \qquad \text{......(9')}.$$

The ratio of the right-hand side of equation (9) to that of equation (9′) differs from 1 by less than $\Sigma \delta_n L / nL_m$. The values of this quantity, derived from $A_n$ on the assumption of linearity over the small range $\delta_n L$, is given in the last column of table 1. They are thus percentage corrections to be applied to the preceding column. It will be seen that the difference between equation (9) and equation (9′) can never lead to an absolute correction amounting to 0·1 in the tabulated value of $(A_n - B_n)/A_n$. Since the tabulated values are subject to an experimental error of this order, we may take them as giving $f(nL_m) - f(L_m)$.

These values may now be plotted against $n$ or $L = nL_m$; see curve 8 of figure 6. The resulting curve defines a function $F(L)$ which has this significance, to our order of approximation. If we calibrate the cell by observing the current $I_c$ due to the known illumination $L_c$ and find $L_c$, equal to $\beta I_c$, then the relation between any known illumination $L$ and the current produced by it is

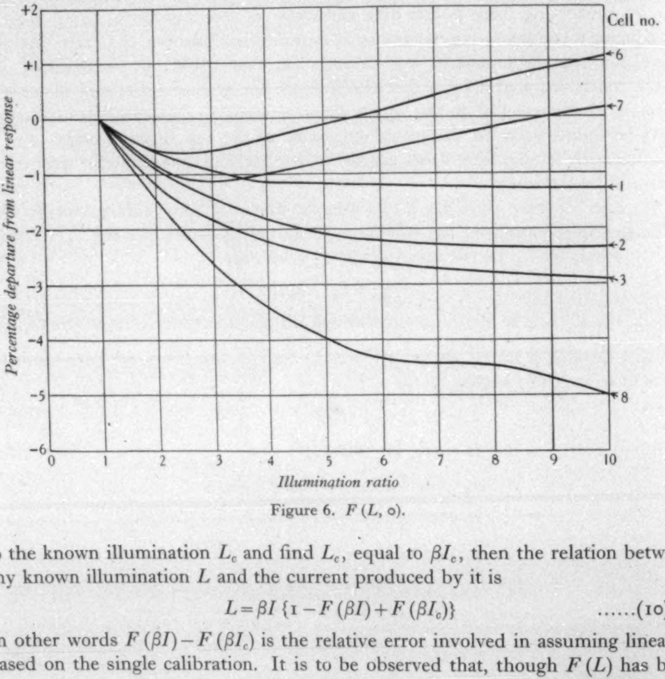

Figure 6. $F(L, 0)$.

$$L = \beta I \{1 - F(\beta I) + F(\beta I_c)\} \qquad \text{......(10).}$$

In other words $F(\beta I) - F(\beta I_c)$ is the relative error involved in assuming linearity based on the single calibration. It is to be observed that, though $F(L)$ has been deduced on the assumption that the calibration is made with the illumination $L_m$, equation (10) is true to our order of approximation whatever the illumination used in calibration, so long as it is such that $f(L)$ is always small. Accordingly, $F(L)$ gives all the information required for the practical purpose of estimating or correcting errors arising from the assumption of linearity.

Strictly $F(L)$ is given only for values of $L$ equal to $nL_m$, where $n$ is an integer from 1 to 10. To obtain $F(L)$ for other values between 0 and $nL_m$ we should have to interpolate, and to extrapolate to zero. The curves are sometimes so irregular that this process, and especially the extrapolation, would be precarious; accordingly, it has not been effected. Of course, the only way to remove this uncertainty is to repeat the observations with a smaller value of $L_m$.

For a general survey of the results obtained with different cells, another procedure is desirable. If we knew the curve relating $I$ and $L$ and intended to assume linearity in our measurements, we should fit to the curve the best straight line, and assume the relation given by this line. The cell would be more linear, the less the average departure of the curve from the straight line. The procedure is not perfectly determinate, because the expression "best straight line" is ambiguous; and it is not worth while to discuss in detail how the procedure might be carried out on the basis of our observations. But consideration will show that an indication of the average departure is given by the quantity $R$, equal to $1-(\Sigma B_n)/(\Sigma A_n)$, where $\Sigma B_n$ and $\Sigma A_n$ are respectively the sums of columns 8 and 5 in table 1. $(1-k)$ is roughly the ratio of the slope of the line through the origin and the point $(I_m, L_m)$, which approximates to the tangent to the curve at the origin, to the slope of the best straight line. $k$ is zero if the curve is linear, or if there is no general tendency of $I/L$ to increase or decrease with $L$; it is positive (or negative) if $I/L$ generally decreases (or increases) with $L$, so that $F(L)$ is predominantly positive (or negative). Accordingly, we may say roughly that the curve is the more linear the less the value of $|k|$. It is to be observed that a positive $k$ implies a normal departure from linearity, such as is known to be due to a large value of $R$; a negative $k$ implies an opposite departure such as has hitherto been regarded as abnormal.

## § 5. RESULTS

The results of our observations can now be summarized in table 2 and figures 6, 7, 8.

In table 2, column 1 gives the numeral assigned to the cell. All the cells were of the selenium, not the cuprous-oxide, type, and all were commercial products; but since we do not know whether their makers would regard them as typical of their manufacture, it would not be proper to give the makers' names. Cell no. 8 was covered with a filter designed to make its sensitivity agree with the luminosity curve; this is why its sensitivity is relatively low. The other cells were bare. Column 2 gives the area of the cell. Column 3 gives the resistance $R_c$ of the cell, measured in the dark with a small current at 30° C.; column 4 gives the temperature coefficient $\gamma$ of the resistance, equal to $1/R_c . dR_c/dT$ and measured over the range 20° to 40° C. $\gamma$ is not always strictly constant over this range, but a mean value, sufficient for our purpose, is assigned. Column 5 gives $R$ in figure 3. Column 6 gives $\sigma$, the sensitivity, measured in microamperes for an illumination of 240 lux ($L_{10}'$) at 30° C. Column 7 is the temperature coefficient $\theta$ of the sensitivity, equal to $1/\sigma . d\sigma/dT$ between 20° and 40° C.; it again is a mean value. Column 8 gives $k$. In figures 6, 7, 8 some of the $F(L)$ curves are plotted as described. Figure 6 collects those for which $R=0$; figure 7 shows how $F(L)$ varies with $R$ in cell no. 8;

figure 8 shows $F(L)$ for that value of $R$ which gives minimum $|k|$ in each cell. It is not shown for cell 6, for which $|k|$ is least when $R=0$.

The measurements on cells 4, 5 are much less satisfactory than the others, for these cells show marked fatigue. It is not easy to obtain measurements to 1 part in 1000 and, if a measurement is obtained, its interpretation is doubtful. The cells are included in order to show the faults of really bad cells. In these cells $F(L)$ is so large when $R=0$ that it cannot be plotted on the scale of figure 6.

Table 2

| Cell no. | Area (cm.²) | $R_c$ at 30° C. (Ω.) | $\gamma$ (per cent per °C.) 20° to 40° | $R$ (Ω.) | $\sigma$ at 30° C. | $\theta$ (per cent per °C.) 20° to 40° | $k$ |
|---|---|---|---|---|---|---|---|
| 1 | 5·4 | 540 | −3·5 | 0 | 42·9 | $-0{\cdot}2_0$ | −0·010 |
| | | | | 200 | — | — | −0·005 |
| | | | | 250 | 26·4 | $-1{\cdot}4_4$ | $+0{\cdot}004_5$ |
| 2 | 5·4 | 1105 | −3·7 | 0 | 32·6 | $-0{\cdot}6_2$ | −0·022 |
| | | | | 100 | — | — | $-0{\cdot}010_5$ |
| | | | | 200 | 24·8 | $-1{\cdot}8_4$ | +0·010 |
| 3 | 5·4 | 2560 | −2·3 | 0 | 42·6 | $-0{\cdot}0_8$ | −0·025 |
| | | | | 200 | 35·8 | $-0{\cdot}5_8$ | $+0{\cdot}003_5$ |
| 4 | $5{\cdot}2_5$ | 9410 | −2·3 | 0 | 45·1 | $+0{\cdot}1_4$ | −0·119 |
| | | | | 250 | — | — | −0·073 |
| | | | | 500 | — | — | −0·045 |
| | | | | 750 | — | — | −0·050 |
| | | | | 1000 | 32·9 | $-0{\cdot}7_8$ | $+0{\cdot}009_5$ |
| 5 | $5{\cdot}2_5$ | 3570 | −2·3 | 0 | 37·9 | $+0{\cdot}5_1$ | −0·280 |
| | | | | 1000 | — | — | −0·092 |
| | | | | 2000 | 17·1 | $-0{\cdot}7_7$ | +0·028 |
| 6 | 5·4 | 4710 | −4·8 | 0 | 63·5 | $-0{\cdot}2_3$ | $+0{\cdot}004_5$ |
| | | | | 100 | 58·5 | $-0{\cdot}3_5$ | $+0{\cdot}012_5$ |
| 7 | 5·4 | 2170 | −4·8 | 0 | 62·0 | $-0{\cdot}5_1$ | $-0{\cdot}003_5$ |
| | | | | 100 | 53·8 | −0·6 | +0·002 |
| | | | | 500 | — | — | +0·065 |
| 8 | 12·6 | 3530 | −3·0 | 0 | 13·8 | $-0{\cdot}0_7$ | −0·042 |
| | | | | 250 | — | — | −0·050 |
| | | | | 500 | — | — | −0·044 |
| | | | | 1000 | — | — | −0·021 |
| | | | | 1250 | — | — | $-0{\cdot}009_5$ |
| | | | | 1350 | $9{\cdot}0_2$ | $-0{\cdot}8_8$ | $+0{\cdot}000_5$ |
| | | | | 1500 | — | — | $+0{\cdot}009_5$ |

The following features of table 2 should be observed: (1) $k$ is usually negative when $R=0$; the departure from linearity is abnormal. As is to be expected, $k$ is always positive for sufficiently large values of $R$. Accordingly there is usually a value of $R$ other than zero at which $k$ is most nearly zero and the cell most nearly linear, according to our criterion. (2) Even when $k$ is most nearly zero, the maximum value of $F(L)$ is always greater than 1 per cent. (3) The value of $R$ which makes $|k|$ least tends to increase with $R_c$; but there is no simple relation between this value and $R_c$ which would enable the value to be found without calibration. (4) $\theta$, the rate of variation of sensitivity with temperature, is always least when $R=0$; as is to be expected, the sensitivity is always greatest when $R=0$.

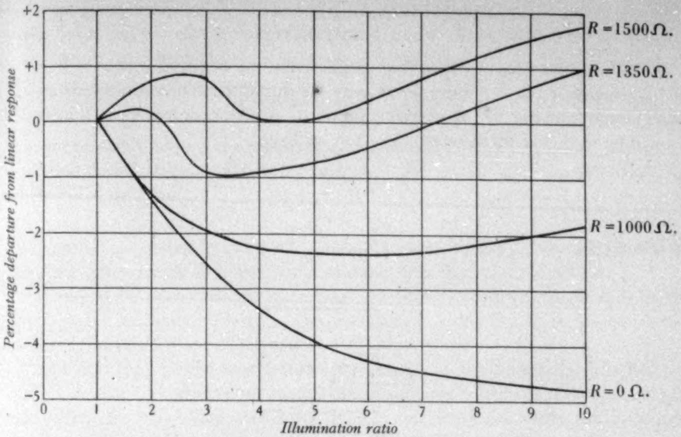

Figure 7. *F* (*L*, *R*) for cell no. 8.

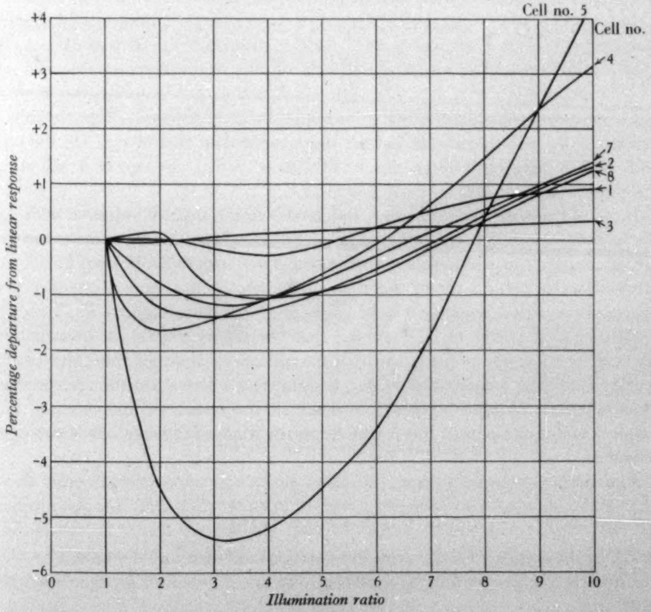

Figure 8. Best *F* (*L*, *R*).

## § 6. THEORY

The simplest way of explaining qualitatively the negative values of $k$ and of $F(L)$ when $R=0$ is to modify figure 1 by the introduction of a series resistance $S$, inseparable from the cell, as shown in figure 9. $S$, like $r$, decreases as $L$ increases, as will be indicated by the suffix $L$. Then we have

$$I=I_0\frac{r_L}{R+r_L+S_L} \qquad \text{......(11).}$$

When $R=0$,

$$\frac{I_0}{I}=\left(1+\frac{S_L}{r_L}\right) \qquad \text{......(12),}$$

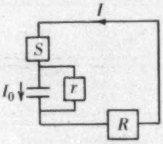

Figure 9.

the sign of $d(I_0/I)/dL$, which is the sign of $k$ and of $F(L)$, will be the sign of

$$\left(\frac{1}{S_L}\frac{dS_L}{dL}-\frac{1}{r_L}\frac{dr_L}{dL}\right).$$

Hence we can explain a negative value of $k$ at $R=0$ by supposing that the relative decrease of $S_L$ with increase of light is more rapid than that of $r_L$. On the other hand, if $R$ is sufficiently large compared with $(r_L+S_L)$, the sign of $k$ will always be that of $-dr_L/dL$, that is to say positive.

If, as perhaps we should expect, differences in the relative variation with light were always associated with differences of the same sign in the relative variation with temperature, it would follow that, when $R=0$, negative $k$ is always associated with positive $\theta$. Table 2 shows that this is not the case, but there is a tendency for more positive or less negative $\theta$ to be associated with more negative $k$.

Of course it is not to be expected that the theory should be quantitatively accurate, for it cannot be really true that the cell can be analysed into three entirely separate elements, a current generator, a shunt and a series resistor. Accordingly, it has not been thought necessary to make all the measurements required for a complete quantitative test. But it may be worth while to indicate how a test might be made.

Equation (11) can be written

$$R=-(r_L+S_L)+r_L I_0\frac{1}{I} \qquad \text{......(13).}$$

Hence, by measuring $I$ for the same illumination (and therefore the same $I_0$, $r_L$, $S_L$) but different $R$'s, and plotting $R$ against $1/I$, we should obtain a straight line whose intercept gives us $(r_L+S_L)$. We know $(r_0+S_0)$, the resistance of the cell in the dark, when $I_0=0$, and its relative temperature coefficient $\gamma$. If we write $\delta$ for the relative temperature coefficient of $r_L$, i.e. $1/r_L \,.\, dr_L/dT$, we have

$$\theta=\delta-\frac{r_L+S_L}{R+r_L+S_L}\gamma_L \qquad \text{......(14)}.$$

We may probably take $r_L$, the temperature coefficient of $(r_L+S_L)$, to be independent of $L$, and therefore may identify $\gamma_L$ with $\gamma$ in table 2. If then $\theta_0$ is the value of $\theta$ when $R=0$, we have

$$\delta=\theta_0+\gamma \qquad \text{......(15)},$$

and $\delta$ can be calculated from table 2. Since we know $(r_L+S_L)$, we can then calculate $\theta$ for other values of $R$ and compare them with the observed values.

It should be observed that we cannot estimate $r_L$ from the slope $r_L I_0$ of the line (13), for we cannot know $I_0$. According to equation (11) $I_0$ is not equal to $I$ for any value of $R$ at any illumination.

Equation (13) is very well fulfilled for cell no. 8 and somewhat less well for cell no. 4. For the other cells the data are insufficient to test the relation. But by calculation, $(r_L+S_L)$ being estimated in all cases from the value for $R=0$ and the other value at which the temperature coefficient of the sensitivity was measured, we arrive at the result shown in table 3, in which the observed and calculated values of $\theta$ are compared. The error in $(r_L+S_L)$ is often very large, but then the effect of an error is correspondingly small. The agreement is by no means perfect, but it is perhaps sufficient to indicate that the theory has some foundation and that there is something of the nature of a series resistance whose variation with temperature and illumination is not the same as that of the shunt resistance.

Table 3

| Cell no. | $(r_L+S_L)$ | $\theta_0$ | $\gamma$ | $\delta$ | $R$ | $\frac{r_L+S_L}{R+r_L+S_L}\gamma$ | $\theta$ calculated | $\theta$ observed |
|---|---|---|---|---|---|---|---|---|
| 1 | 397 | $-0{\cdot}2_0$ | $-3{\cdot}5$ | $-3{\cdot}7$ | 250 | $-2{\cdot}1_5$ | $-1{\cdot}5_5$ | $-1{\cdot}4_5$ |
| 2 | 639 | $-0{\cdot}6_2$ | $-3{\cdot}7$ | $-4{\cdot}3$ | 200 | $-2{\cdot}8$ | $-1{\cdot}5$ | $-1{\cdot}8$ |
| 3 | 1070 | $-0{\cdot}0_8$ | $-2{\cdot}3$ | $-2{\cdot}3_8$ | 200 | $-1{\cdot}9_4$ | $-0{\cdot}4_5$ | $-0{\cdot}6$ |
| 4 | 2680 | $+0{\cdot}1_4$ | $-2{\cdot}3$ | $-2{\cdot}1_6$ | 1000 | $-1{\cdot}6_8$ | $-0{\cdot}5$ | $-0{\cdot}8$ |
| 5 | 1640 | $+0{\cdot}5_1$ | $-2{\cdot}3$ | $-1{\cdot}8$ | 2000 | $-1{\cdot}0_4$ | $-0{\cdot}8$ | $-0{\cdot}8$ |
| 6 | 1180 | $-0{\cdot}2_3$ | $-4{\cdot}8$ | $-5{\cdot}0$ | 100 | $-4{\cdot}4$ | $-0{\cdot}6$ | $-0{\cdot}3_5$ |
| 7 | 657 | $-0{\cdot}5_1$ | $-4{\cdot}8$ | $-5{\cdot}3$ | 100 | $-4{\cdot}2$ | $-1{\cdot}1$ | $-0{\cdot}6$ |
| 8 | 2540 | $-0{\cdot}1$ | $-3{\cdot}0$ | $-3{\cdot}1$ | 1350 | $-1{\cdot}9_5$ | $-1{\cdot}1$ | $-0{\cdot}9$ |

## § 7. PRACTICAL CONCLUSIONS

If measurements cover the whole range from 0 to 240 lux, and if it is assumed that the cell is linear, then errors of at least 1 per cent will be made, whatever the illumination at which the cell has been calibrated and whatever the value of the external resistance $R$. In a bad cell the errors may amount to 5 per cent, even with the best value of $R$. Cells with high internal resistance are not in general better in respect of linearity than cells with low internal resistance. The experiments are not sufficient to determine what errors are introduced by assuming linearity over some lower range of illumination, say 0 to 20 lux. But they do not suggest that, even over that range, the errors will be less than several parts in 1000.

If an accuracy better than 1 per cent from 0 to 240 lux is required, the cell must be calibrated by addition, as described above, over the whole range. The calibration must be repeated at least once a day. It is then best to make $R$ equal to 0 and to use the compensation circuit of figure 3, for the effect of temperature variations is then least. If linearity is to be assumed, then the errors arising from that assumption will generally be least when $R$ has some value between 100 and 1500 Ω., which can only be determined by a full calibration. But the value is substantially constant for a given cell over considerable periods of time. However, use of this value of $R$ will increase the temperature variations materially.

For a given value of $R$ there is no difference between the behaviour of the circuits of figure 1 and figure 3 except in respect of sensitivity. The circuit of figure 3 is always the more sensitive, and permits the use of a resistance $R$ smaller than is practicable with the circuit of figure 1. If a null method of measuring the current is to be used in any case, the circuit of figure 3 has no compensating disadvantage, but that of figure 1 has the advantage that it can be made direct-reading. If linearity is to be assumed, the superior accuracy of null methods is illusory; the ordinary direct-reading circuit shown in figure 1 is then as good as any other, unless temperature variations are likely to be so serious that it becomes important to make $R$ equal to 0.

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# A PROBLEM IN COIN-TOSSES

### By S. R. SAVUR

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*ABSTRACT.* The paper discusses data given by Norman Campbell, on the tossing of a coin, and questions his conclusion that the particular series quoted does not define a chance. A theoretical investigation is made of the limits within which the heads-to-tails ratio obtained by tossing a coin a given number of times may be expected, to diverge from unity, and a practical study of the drawing of coloured balls from a container is described. The conclusion reached is that the ratio obtained in practice converges towards the ratio to be expected on theoretical grounds provided that the number of trials is very great indeed.

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In his paper "The statistical theory of errors"(1) Norman Campbell has tried to show that statistical methods are irrelevant to "problems that arise in drawing conclusions from observations". It is not the purpose of this short note to discuss how far Campbell has succeeded in proving his point, but rather to consider a conclusion drawn by him, namely, that the application of statistical methods to the experimental results obtained even in a problem "to which every one admits statistics to be relevant" does not give definite results.

The problem considered by Campbell is the tossing of a coin. His comments, pp. 802–3, on the results of the experiments are: "It is unusual to have as many as 20 measurements made in conditions so similar that, on any reasonable theory of error, they can be regarded as a sample drawn from a single collection. How inadequate such a sample is to prove the existence of a chance is illustrated by figures 1 and 2. These show the results of 500 tosses of a coin recorded for this occasion.... The first 20 tosses suggest no characteristic chance; the first 90 suggest one definitely greater than 0·5; the first 500 leave the whole matter doubtful. Many more tosses would be required to convince anyone that the chance really exists."

The impression one gets from these remarks is that although 20 experiments are usually believed to be sufficient by statisticians, yet even a number of experiments (or tosses) as high as 500 in the case of a coin, to which statistical methods are undoubtedly applicable, has been found insufficient for the purpose, and that hence there must be something wrong in statistical methods as advocated at present.

This view is not correct because it depends upon the assumption that about the same number of experiments will be required in all cases, an assumption that does not appear to be reasonable. The following illustration will make this point clearer. Suppose we are measuring a certain length. The errors in our measurements are assumed to be normally distributed with a standard deviation much less than the length we are trying to measure. Hence 20 experiments (or measurements) may be believed to be sufficient by many persons. In the case of the tossing of a coin, however, the distribution is Bernoullian. Hence not only should we not expect that 20 experiments (or tosses) will suffice in this case too, but we should also not be surprised if the required number comes out to be unsuspectingly bigger than 20. We will now show what can be deduced from the results of tossing a coin a given number of times.

Suppose we toss a coin $n$ times and observe that heads have turned up $l$ times. Two courses are open to us: (1) we can see whether we could have got by random chance alone $l$ heads in $n$ throws assuming that the coin is true; or (2) we can calculate the limits between which lies the chance of getting a head in a random throw of that coin.

Let us take course (1) first. Suppose $l \leqslant \frac{1}{2}n$. (If $l > \frac{1}{2}n$ the procedure to be adopted is the same but tails are used instead of heads.) Then the chance of getting $l$ or a smaller number of heads in $n$ throws is given by

$$P = I_{\frac{1}{2}}(n-l,\ l+1) \qquad \ldots\ldots(1),$$

where $I_{\frac{1}{2}}(n-l,\ l+1)$ is an incomplete $\beta$-function ratio written after the manner of Karl Pearson[(2)]. (i) If neither $n-l$ nor $l+1$ is greater than 50, we can use Pearson's tables[(2)] to evaluate $P$ in equation (1). (ii) When either $(n-l)$ or $(l+1)$ or both are greater than 50, we can use a very approximate method which depends upon the fact that the binomial distribution $(\frac{1}{2}+\frac{1}{2})^n$ approaches the corresponding normal distribution very rapidly with the increase in $n$. It can easily be shown that $P$ is approximately equal to $\{1-\frac{1}{2}(1+\alpha)\}$, where $\frac{1}{2}(1+\alpha)$ corresponds to $x=(n-2l)/\sqrt{n}$ and can be found from table II of reference (3); $\alpha$ and $x$ have the same meaning as in that table.

It has been verified in a large number of cases, to every one of which method (i) could be applied, that the value of $P$, when not less than 0·001 (i.e. for a range of values which are sufficient for all practical purposes), calculated from (ii) is slightly less than the correct value of $P$ obtained from (i). Almost the whole of this difference appears to be due to the fact that whereas (i) gives the sum of the first $l+1$ terms of the binomial $(\frac{1}{2}+\frac{1}{2})^n$, method (ii) gives, as we might expect, the sum of the first $l$ terms $+\frac{1}{2}$ of the $(l+1)$th term of the same binomial. With this correction we get

$$P = 1 - \{\tfrac{1}{2}(1+\alpha) \text{ corresponding to } x=(n-2l)/\sqrt{n}\} + \frac{n!}{2^{n+1}(n-l)!\,l!} \qquad \ldots\ldots(2).$$

The necessity for this correction may also be seen from the Euler-Maclaurin formula for replacing a sum by an integral.

Let us select some limit for random chance, say $P_1$. Then we consider that our sample of $n$ throws containing $l$ heads could have been obtained by random chance from a true coin if $P \nless \frac{1}{2}P_1$; the factor $\frac{1}{2}$ is used because we want to treat on the same footing heads and tails obtained from the tossing of a true coin.

We will consider first an example to which both the methods (i) and (ii) are applicable.

*Example* 1. Out of the first 90 throws of a coin made by Campbell 48* were heads. Could this result have been obtained from a true coin by random chance? Since $48 > \frac{1}{2} \times 90$, we will use the tails instead and see whether we could have obtained by random chance 42 tails in 90 throws of a true coin. Suppose we select 5 per cent as our limit for random chance. Then $P_1 = 0{\cdot}05$. Let us use method (i) first. From equation (1)

$$P = I_{\frac{1}{2}}(48, 43) = 0{\cdot}2992,$$

from Pearson's tables$^{(2)}$.

Let us now use method (ii). We have

$$x = (90 - 2 \times 42)/\sqrt{90} = 0{\cdot}6324.$$

From tables II of reference (3), $\frac{1}{2}(1 + \alpha)$ corresponding to $x = 0{\cdot}6324$ is $0{\cdot}7369$. Hence from equation (2) we get

$$\begin{aligned} P &= 1 - 0{\cdot}7369 + \frac{90!}{2^{91} 48!\, 42!} \\ &= 0{\cdot}2975. \end{aligned}$$

This differs from the true value by 0·0017 only, or by less than 0·6 per cent, showing that method (ii) is quite a good method. Since $P \nless \frac{1}{2} P_1$ we see that the result of the first 90 throws obtained by Campbell could have been obtained by random chance from a true coin. We will now apply method (ii) to the whole of the 500 throws made by Campbell.

*Example* 2. 260 heads were obtained in 500 throws. Could this result have been obtained by random chance alone with a true coin? As before we will consider the case of 240 tails in 500 throws. We have to apply method (ii) here. Proceeding as explained in example (1) we get $P = 0{\cdot}1982$. We thus see that the result could have been obtained with a true coin on the basis of our limit of 5 per cent for random chance.

We have been considering so far that the coin is true. We will now adopt the other course, namely, to find out the nature of the coin from the results of throws. Let us assume as before that in $n$ successive throws of a coin $l$ heads turned up. Suppose the chance of a head turning up in a random trial is $k$. Then it is shown in reference (4) that

$$k_2 \leqslant k \leqslant k_1,$$

where $k_2$ and $k_1$ are given by

$$\left.\begin{aligned} P_1 &= I_{k_2}(l, n - l + 1) \\ \text{and} \qquad P_1 &= I_{1-k_1}(n - l, l + 1) \end{aligned}\right\} \quad \ldots\ldots(3),$$

the $I$'s being incomplete $\beta$-function ratios. $P_1$ is the limit for random chance selected by us. When neither $(l + 1)$ nor $(n - l + 1)$ is greater than 50, it is possible to use a method of evaluating $k_1$ and $k_2$ given in reference (4), but for higher values the method of solution is rather tedious and laborious. So we shall apply it to the case where neither $(l + 1)$ nor $(n - l + 1)$ is greater than 50, that is to the case in example (1) above.

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* This number, although determined from figure 1 on p. 802 of Campbells' paper$^{(1)}$ appears to be correct.

*Example* 3. When a coin was tossed 90 times, the number of heads that turned up was 48. What is the chance of a head turning up in a random trial? Let us choose 5 per cent as our limit for random chance. Then the equations (3) become

$$0{\cdot}05 = I_{k_2}(48, 43)$$

and

$$0{\cdot}05 = I_{1-k_1}(42, 49).$$

Solving these in the manner described in reference (4) or otherwise we get $k_2 = 0{\cdot}39$ and $k_1 = 0{\cdot}62$. Hence the chance of a head turning up is some fraction between 0·39 and 0·62, these limits being included. From the sample, the chance of a head turning up is $48/90 = 0{\cdot}533$, and is, naturally, between the limits obtained above.

Coming now to p. 802 of Campbell's paper(1) we see that he distinguishes between two sorts of "collections", to which the application of statistics is admitted to be relevant by him. These, according to him, "are typified respectively by the urn containing black and white balls and the die or the tossed coin". The first sort being finite and enumerable it is possible to know accurately the proportion of members of the different kinds in it. With regard to the second sort he says that it "is not finite or even definite, it is impossible that this proportion should ever be known accurately". His experiment of 500 tosses with a coin was designed to see how many tosses would be required to determine with some accuracy the chance of a head turning up in a random trial. His conclusion stated above is that many more than 500 tosses would be required "to convince anyone that the chance really exists".

Campbell's idea appears to be the following. If the chance of a head turning up when a coin is tossed has a definite value, then this chance calculated from the results of a number of throws of the coin should fluctuate about this definite value as the number of throws is increased, in a manner somewhat similar to, though not so regular as, that of, say, the fluctuations of a dying oscillatory current. Since his figure 2 does not show the least sign of the chance tending to a definite value even when the number of throws is increased to 500, he was led to doubt even the existence of a definite value of the chance. To test Campbell's method of reasoning the following experiment was performed.

Eight marbles of the same size, 4 white and 4 coloured, were put in a cylindrical vessel *A* which was capable of rotation by means of a handle *C*, figure 1. *A* had a circular opening slightly bigger than that required for the marbles to be put in one by one. This opening could be closed by a lid *B*. A draw was made in the following manner. After the opening has been closed *A* is rotated a few times to ensure thorough mixing of the balls. The opening is brought down and the lid pushed aside slowly so that only one marble is allowed to come out. The colour of the marble is noted. This is a draw. For another draw this marble is put back into the vessel, the lid is closed, the vessel is rotated a small number of times and then a marble is drawn out as before. Since there are 4 white and 4 coloured marbles every time before a draw is made, the chance of drawing a white marble is exactly $\frac{1}{2}$. The results of 600 draws are shown in figures 2 and 3.

In these figures the proportion of the number of white marbles drawn in a

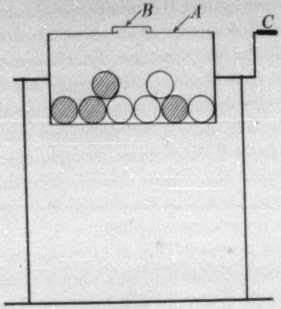

Figure 1.

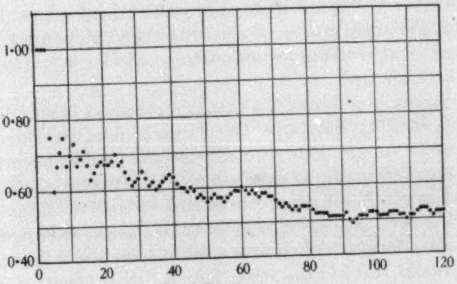

Figure 2.

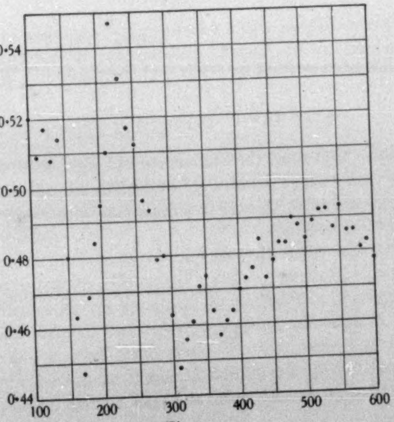

Figure 3.

number of draws is plotted against the number of draws. Campbell's method of reasoning leads us to the deduction that the first 140 draws suggest a chance definitely greater than $\frac{1}{2}$ and the first 600 leave the whole matter doubtful. This is the conclusion reached in the case where we are certain that the chance is definitely $\frac{1}{2}$!

The fallacy in Campbell's method of reasoning lies in the fact that the variations due to random sampling have been overlooked and that their effects, shown in figures 1 and 2 of his paper(1), have been wrongly interpreted as not pointing unequivocally to the existence of a definite chance. In reality his figures 1 and 2 and figures 2 and 3 of this paper should be considered to be quite good illustrations of the fluctuations met with in random sampling.

It is believed, or rather assumed, that in cases similar to those which we have considered above the chance calculated from a sample tends to a limit, which is the true chance, when the sample is increased indefinitely. Two proofs of the accuracy of this assumption have been given by Dorothy Wrinch and Harold Jeffreys(5) and by M. S. Bartlett(6). The following proof in which the variations due to random sampling have been taken into account may be found interesting. We have considered the case in which the true, or population, chance is $\frac{1}{2}$. This proof can be adapted to other cases also.

Suppose a sample of $n$ throws of a true coin contains $l$ heads. We will assume, without loss of generality, that $l \leqslant \frac{1}{2}n$. The chance of obtaining a head is thus $l/n$ as calculated from the sample. This value is subject to random fluctuations. Hence we will restrict ourselves only to those samples, of $n$ random throws each, which could have been obtained with a true coin by random chance. Of this group we will consider only the worst sample, worst in the sense that the chance calculated from it has the greatest difference from the true value. Clearly it is that sample in which $l$ has the smallest value. Let $l_1$ be this value. Then if $P_1$ be the limit for random chance selected by us, we have, from equation (2) above,

$$\tfrac{1}{2}P_1 = 1 - \tfrac{1}{2}(1+\alpha)_x + \frac{n!}{2^{n+1}(n-l_1)!\,l_1!},$$

where $\frac{1}{2}(1+\alpha)_x$ has been written for "$\frac{1}{2}(1+\alpha)$ corresponding to $x=(n-2l_1)/\sqrt{n}$". Hence

$$\tfrac{1}{2}(1+\alpha)_x = 1 - \tfrac{1}{2}P_1 + \frac{n!}{2^{n+1}(n-l_1)!\,l_1!} \qquad \text{......(4).}$$

It has been shown above that the last term on the right-hand side of equation (4) is small in comparison with $\frac{1}{2}P_1$, and since we usually assume that $P_1 = 0{\cdot}05$ or less, that term will be considerably smaller in comparison with $1-\frac{1}{2}P_1$. This term being neglected, equation (4) becomes

$$\tfrac{1}{2}(1+\alpha)_x = 1 - \tfrac{1}{2}P_1 \qquad \text{......(5).}$$

Using tables II of reference (3) we can find the corresponding value of $x$. Let $x_1$ be this value. But

$$x_1 = \frac{n-2l_1}{\sqrt{n}},$$

$$\therefore \quad \tfrac{1}{2} - \frac{l_1}{n} = \frac{x_1}{2\sqrt{n}} \qquad \text{......(6).}$$

The left-hand side of equation (6) gives the biggest difference that can be expected on the basis of random chance between the true value of the chance and that calculated from our sample. From equation (6) we see that this difference decreases indefinitely as $n$ increases without limit. Hence the chance calculated from a sample tends to the true value in the limit when the sample is increased indefinitely.

Let us denote by $d_m$ the maximum difference obtained on the basis of random chance between the true chance and the chance calculated from a random sample. Equation (6) may now be rewritten as

$$d_m = \frac{x_1}{2\sqrt{n}} \qquad \text{......(7).}$$

If we use 5 per cent as our limit for random chance, $x_1$ comes out as 1·96. Hence equation (7) becomes

$$d_m = 0{\cdot}98/\sqrt{n} \qquad \text{......(8).}$$

Suppose we want to know how big the sample should be in order that the chance calculated from it may be correct to within ±1 per cent. This means that $d_m$ should not exceed 0·005. From equation (8) we see that

$$n \not< \left(\frac{0{\cdot}98}{0{\cdot}005}\right)^2, \text{ i.e. } \not< 38{,}400.$$

Is it surprising then that Campbell's 500 tosses or the 600 draws in the experiment described in this paper were quite inadequate in comparison with the nearly forty thousand experiments that are required?

Let us now apply equation (8) to the experimental result obtained by Campbell, namely that in 500 tosses 260 heads were registered. We have

$$d_m = \frac{0{\cdot}98}{\sqrt{500}} = 0{\cdot}044.$$

The actual difference obtained by Campbell is 0·020, which is less than the maximum value that may be expected from random chance. In the case considered in this paper $n = 600$, so that

$$d_m = \frac{0{\cdot}98}{\sqrt{600}} = 0{\cdot}040.$$

The actual difference observed is 0·022, which is again below the maximum value. Hence we conclude that the samples obtained by Campbell and by us could have been obtained from their respective hypothetical populations by mere chance.

It will be clear from the above that statistical methods, when properly applied, have given us quite definite results in the cases in which Campbell found them to lead to inconclusive results. Campbell's deductions have already been shown to be due to an incorrect interpretation of the experimental values obtained by him and not to any faultiness in the statistical methods themselves. Besides, there do not appear to be any a priori reasons why in other cases also, to which statistical methods are applicable, these methods should not give definite results. Hence Campbell's other point, namely, that it is illegitimate to apply statistical methods to "problems that arise in drawing conclusions from observations", requires justification. I intend to examine this point in some detail on a future occasion.

In conclusion I wish to thank the referee for having kindly drawn my attention to the two papers (5) and (6).

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## DISCUSSION ON PROBABILITY, BASED ON THE PRECEDING PAPER

Mr J. H. AWBERY. In the absence of the author, it falls to me to open this discussion, which as you know is to be a discussion on a field larger than that covered by Savur's paper. I shall take it upon myself to suggest later just how wide it should be.

The paper with which we open arises from one by N. R. Campbell, which in its turn arose from one by Deming and Birge, into the details of which I will not ask you to enter now. But we must consider Campbell's. He was concerned to show that the adjustment of observations is not a subject to which statistics can be applied, because a collection of physical observations is not a sample from some larger collection of measurements-that-were-not-and-never-will-be-made. To illustrate his claim that 20 observations (but note that this word is interpreted in different ways by Campbell and by Savur) are not sufficient to prove the existence of a chance, Campbell gave the results of tossing a coin not 20, but 500 times, and considered that the results do not prove the existence of a chance even here. (Of course, he admits that there *is* a chance, in the neighbourhood of 0·5, but points out that this series does not suffice to prove it.) It is this point, and not his general claim, which is subjected to analysis by Savur, to whose paper we therefore turn.

In the first place, Savur mentions that the number of observations necessary to assess a chance will depend on the nature of the series from which the sample is taken, and in particular on its dispersion. He then proceeds to calculate the chance of obtaining, say, $l$ heads in $n$ throws, and obtains a precise result, which can be evaluated from tables over a certain range of the two variables involved. Outside this range, he contributes a new approximation formula. The author then examines Campbell's results, and finds that they are consistent with a chance of 0·5. Later, he calculates the reverse problem, how many tosses would be necessary to deduce the chance within 1 %, and finds the answer, 38,000. He thus agrees with Campbell that many more than 500 are necessary. Finally, he gives experiments on the drawing of balls from a container, where the chance (the collection being finite) is not liable to be disputed, and shows that the results, like Campbell's, would fail, on a cursory examination, to suggest any "chance".

His paper also contains another original section, in which a new proof is offered of the proposition that under certain conditions, the chance calculated from a sample tends to a limit, which is the true chance, as the size of the sample is indefinitely increased.

I have tried to give briefly the relation between Campbell's and Savur's papers, and to point out that there are several problems. I suggest that we leave aside the one, "Are statistical methods relevant to the adjustment of observations?", and concentrate on "What information, and with what degree of certainty, can be obtained by examining the results of selecting $N$ objects from a collection which is known to be infinite?"

Dr N. R. CAMPBELL. The author has misunderstood me. I did not assert that ordinary statistical methods were not valid in their own field, or that this field did not include the tossing of a coin. What I said was that they are not applicable to the field that I was discussing, namely the adjustment of divergent measurements of the same magnitude. I said further that one (not the only) reason why they are not so applicable was that the number of observations was not sufficient to determine whether the events were random in the sense in which (in my opinion) events must be random if statistics are to be applicable to them.

How irrelevant Mr Savur's paper is to such a thesis will appear from this example. At 100 tosses I had observed 50 heads and 50 tails. Mr Savur's calculations would make his $P$ a maximum for that distribution. But suppose that the sequence had been regularly heads, tails, heads, tails.... That would be, in my opinion, conclusive proof that the events were not random. The evidence on which Mr Savur concludes most definitely that the chance is $\frac{1}{2}$ is the evidence on which I conclude most definitely that there is no chance concerned at all. The reason for the discrepancy is, of course, that Mr Savur assumes what I am concerned to establish, namely that the events are random.

I do not regard the position as materially changed if drawings from an urn are substituted for tosses of a coin. The fact that it is known that there were originally equal numbers of equal balls in the urn does *not* prove that the drawings must be random. For if, during the shaking of the urn, one set of balls wore more rapidly than the other, so that they became smaller, the chance of drawing a ball of this set would progressively increase.

I therefore offer no opinion on the validity of Mr Savur's conclusions; I merely say that they are wholly irrelevant to the particular problem that I was discussing.

Sir ARTHUR EDDINGTON said that the fact that a coin does not come down heads, tails, heads, tails...is evidence for the results being random. He considered what can be inferred from a series of coin-tosses. If we tossed $10^{10^{10}}$ times we should learn something about the character of the coin or the manner of tossing, but nothing about probability. Any deviations from the probable result we should interpret as evidence of asymmetry in the coin. We might examine the coin independently and find it symmetrical, but in that case we might say that the asymmetrical result was due to the volition of the coin itself; if a human being were tossed he would come down feet first oftener than head first. Or more probably we should attribute the result to the experimenter, who must be free to make experimental errors—in this case by failing to toss uniformly. (If Dr Campbell tried to invent ways of making errors instead of the reverse he might produce even stranger results.) There is no general law of errors. If we try to locate a clock pendulum by observing its instantaneous position we obtain a law of error of a kind opposite to the Gaussian law, with fewest observations in the middle of the range. The law of error applicable in each case that arises in practice must be found from theory or experiment. A large number of sources of small error gives the Gaussian law, but systematic errors give other laws. It may be noted that the method of least squares is justifiable for any symmetrical law of error, and not only for the Gaussian law.

Prof. H. Dingle. My remarks relate to the comparison between coin-tossing and physical experiments. By coin-tossing I mean an operation, not a pure calculation. Both theoretical and practical treatments of probability are important, but they should not be confused. The question, "Does a chance exist?" calls in the former for an existence theorem and in the latter for an action. Mr Savur confuses these demands when, speaking of his experiment, he says on theoretical grounds that a chance of $\frac{1}{2}$ exists, finds by trial no chance of $\frac{1}{2}$, and concludes that the theory is applicable to the experiment. The scientific method is not to assert the existence of something and then look for it, but first to look and then ask if the observations justify the assertion that something exists. That requires a criterion of existence, and I suggest that an experiment reveals the existence of something significant if it gives information about the result of repeating it. Thus, if we weigh a solid, the sum of the numbers on the weights which produce balance is approximately reproduced in a second weighing, and we can therefore deduce the existence of something called "weight", which is measured thereby. With that criterion a coin-toss gives no evidence of a physical existence because, no matter how many times we repeat it, we are still no wiser than before about the result of the next toss. A coin-toss is thus not a physical experiment. But a set of $n$ (say 500) tosses does reveal something significant, because experience shows that the proportion of heads in it enables us to say that the proportion of heads in the next set will lie within a range small compared with the possible range (0 to 1). The observed proportion thus represents (perhaps indirectly) a scientific existence which we may call a "chance". A set of $n$ tosses is thus similar to a physical experiment such as weighing, in that a number of performances gives results showing small deviations from their mean value. Care is necessary, however, in pursuing the comparison, because the source of the deviations is differently located in the two cases. In coin-tossing the instructions (concerning the definition of tossing) are imprecise but can be carried out precisely, whereas in weighing the instructions are precise but can be carried out only imprecisely. Consequently the good experimenter's instinct to make experiments as precise as possible in every respect produces startlingly different results. A strictly specified tossing machine would give heads (or tails) every time, so that the "chance" would be changed from, say, 0·5 to 1 or 0, whereas if we had a perfect balance, could read the position of the pointer exactly, etc., the "weight" would be only slightly changed.

Dr Campbell's theory of measurement* does not seem general enough to cover all cases, for the range $\beta$ as defined does not always lie within the range $\alpha$. Thus, in measuring a small stellar parallax known to be positive, $\alpha$ might range from 0″ to 1″, whereas $\beta$ would include small negative as well as positive values. This is another example of the failure of a single observation to pass as an experiment, for otherwise the astronomer would be guilty of accepting results known to be impossible and rejecting possible but abnormal ones.

Capt C. W. **Hume**. I once tossed a man double or quits for a penny stamp, on winning tossed him again, and continued till he owed me £1. 1*s*. 4*d*. If Dr Campbell is right in saying that a regular sequence of heads, tails, heads, tails... "would be conclusive proof that the events were not random", I begin to feel uncomfortable.

Colonel J. L. P. **Macnair**. A discussion of this nature is a little dangerous because so many people are discussing different things. The non-scientific user looks to physicists to guide him in his practical problems. I was a little disturbed, for example, by a statement of Prof. Eddington that "you can't determine much from 20 observations of anything". [Prof. Eddington interposed to say that he was referring principally to astronomical observations.] In the science of ballistics and gunnery we build a very useful practical edifice on the results of 5 observations. It is true that those 5 observations are supported by further series obtained under different conditions, but the latter only increase the certainty; they do not invalidate the results reached with a smaller number. In contrast take the example, already mentioned in this discussion, of the determination of the velocity of light. If we look at this over a number of years it seems within the bounds of possibility that we are measuring something which is not really a constant at all. In that case it may well be that the longer we go on observing it, the greater will be the uncertainty.

Mr T. **Smith**. It is unnecessary to discuss the author's attitude to Campbell's paper, for it is clearly mistaken. Omitting certain parts of physics where statistical theory is of special significance, I find myself in general agreement with Campbell. Among physicists statistical theory has perhaps been most extensively used to derive a better figure of accuracy than the observations will stand. This is an error of youth; later on, when the experimenter has to be prepared to justify his figures against those of all comers he is usually wise enough to leave such indiscretions behind him. The calculations in this paper illustrate that even very many observations tending closely to a certain result are consistent with facts corresponding to values substantially different. In fact the paper shows how necessary for the physicist some such view as Campbell's is.

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* *Proc. Phys. Soc.* **47**, 804 (1935).

Two points in the paper call for comment. In the first place there is no essential difference between the two experiments. In saying that the chance of drawing a white marble is exactly ½ the author is counting the marbles just as with the coin he is counting the faces. This fraction must not be confused with the chance the experiments are supposed to reveal, which is the frequency with which certain events occur.

The other point is that we are never concerned with infinity in these questions; any argument in which infinity occurs is of no interest to us. If we considered an infinitely great number of repetitions, we might well argue that in an infinite collection every possible relation between one group and the other would occur at some stage, including those in which the number of heads was negligible compared with the tails and vice versa. I think it was Sir James Jeans who suggested that, given long enough, a group of monkeys provided with typewriters would reproduce all the books in the British Museum.

The author's theoretical discussion appears to be circular in character, but even so he surely claims more than he is entitled to claim. The chance that the ultimate value of his chance (if this has a meaning) will differ substantially from ½ is merely small.

WILFRED W. BARKAS. I understood Dr Campbell to say that if the sequence came heads, tails, heads, tails, for twenty throws he would reject that series in deducing the chance because it would be possible to suppose some ordered outside influence which could contribute to that result. Since this sequence is a possible result of a truly random experiment, it is just as probable as any one other sequence which Dr Campbell would accept as a suitable sequence on which to calculate the "chance", so it seems to me he is not justified in discarding it and further that an ordered influence might equally well be postulated which would give any sequence which occurred in practice. It seems therefore that it must be the method of calculating the chance which is defective, and this to exactly the degree in which the acceptance of a sequence has to be left to the prejudice of the experimenter. As Mr Smith pointed out, in an infinite series of truly random tosses all possible sequences, including the above, would appear, so a method of calculating the chances should be found which will deal equally well with any of them. The certainty attached to any calculated chance would presumably vary with the number of tosses in the sequence, but not with the order within the sequence.

Mr C. L. T. GRIFFITH. In connexion with the subject of the papers by Mr Norman Campbell and Mr S. R. Savur on coin-tossing it may be mentioned that I made a set of 50,400 draws of a ball from a bowl containing one red, one white and one blue ball, and gave the record to Prof. Egon Pearson at University College, London. The series has been fully abstracted, showing the number of reds in each 10; the number of reds, whites and blues in each 60, 360 etc. sequences; and the progressive deviation from the expected mean for each ball from the start to the end. A diagram is given of these progressive deviations, and has on it the parabola showing the progressive probable errors (not the standard errors). This seems to be a more expressive diagram than those showing the percentage deviations. Nearly 50 years ago I made many experiments for Karl Pearson which he made use of for his Gresham lectures, and I hope that my more recent and extensive tossing may be used to decide some of the questions considered in the two papers under discussion.

Reply by Mr S. R. SAVUR. I wish to express my warmest thanks to Mr Awbery for having read my paper.

With regard to the general point raised in the discussion, I submit there is fundamentally no difference, from the statistical point of view, between the determination of, say, the length of an object by measurement and that of the chance in the tossing of a coin. In the former case, the various measurements differ among themselves and from the true value, which is assumed to remain constant unless there is a reason to the contrary, on account of unavoidable errors of observation, of unknown magnitude. In the second case the observed values differ from the true chance by unavoidable errors due to random sampling. As it is impossible to find the true value, the aim of the statistical theory in both cases is to use the available results to evaluate, upon certain assumptions, the smallest interval within which the true value lies. The theory asserts that this interval diminishes with the increase in the number of observations and becomes zero when the number is infinite. In my paper I have tried to prove the accuracy of this statement in a special case. This does not, however, mean that statisticians are unpractical enough to demand an infinite number of observations even in a single case; this would be my answer to Mr Smith's remark about infinity. Again, I do not agree with him when he says that the chance, $\frac{1}{2}$, obtained from a count of the marbles in my experiment is different from the chance that the experiments are supposed to reveal.

In his paper(1) Dr Campbell doubted the validity of the application of statistical methods to the adjustment of observations and suggested another method. He also discussed the results of an experiment of 500 tosses of a coin to which, according to him, the application of statistics, although relevant, did not give a definite answer. It appeared to me that I should show first that statistical methods do yield a definite answer, though not the one expected by Dr Campbell, in the case which was considered by him and to which they are applicable. It is only after some such justification that one can try to justify the use of statistical methods in other cases to which their applicability is not immediately obvious. For this reason, therefore, I cannot agree with Dr Campbell's final remark.

Dr Campbell is not right when he says that I conclude "most definitely that the chance is $\frac{1}{2}$" on the evidence on which he concludes "most definitely that there is no chance concerned at all". All I have concluded is that the results of both experiments are compatible with the assumption of a chance $\frac{1}{2}$ remaining constant throughout; Mr Awbery's remarks are relevant to this point. Similarly, Prof. Dingle's humorous remark that "Mr Savur says on theoretical grounds that a chance of $\frac{1}{2}$ exists, finds by trial no chance of $\frac{1}{2}$, and concludes that the theory is applicable to the experiment" gives a wrong impression about the method used and the conclusions arrived at by me.

It need hardly be mentioned that a "strictly specified tossing machine", visualized by Prof. Dingle, will never be used, even if available, in those cases in which a decision to be made depends upon the result of a toss. For the interest mathematical or otherwise, in the results of tosses as ordinarily carried out is due just to the impossibility of accurately predicting the result of each toss and to the view that in the long run heads and tails will turn up equal numbers of times.

# DISCUSSION ON ELECTRON-DIFFRACTION AND SURFACE STRUCTURE

*held at a joint meeting of the Physical Society and the Chemical Society at Burlington House, 17 March 1938*

*Abstract of opening address by* Prof. G. I. Finch, F.R.S. The physical and chemical properties of a solid or liquid depend much on the nature and arrangement of the surface atoms, about which electron-diffraction can give direct information, while as a rule other methods of examination only enable inferences to be drawn. The experimental and interpretative techniques have reached a stage which makes electron-diffraction a powerful means of attack on a wide variety of surface problems, some of which are outlined below.

Mechanical working reduces crystal-size, and, under certain conditions, leads to the formation of amorphous surface films. It is shown that Beilby's view, based on microscopic evidence and according to which the polish layer is amorphous both on metals and on non-metals, is not always correct, since in certain cases the flowed material crystallizes. Evidence afforded by a study of isomorphous overgrowths as to the structure of polish on calcite surfaces is in agreement with that afforded by electron-diffraction. The study of the structure of the polish layer yields information of practical interest to the engineer in connexion with mechanical wear.

Experiments on corrosion show that an amorphous surface film confers as a rule greater protection than a crystalline one. As examples, the oxide films on aluminium, silicon carbide and iron are discussed.

In many instances, electron-diffraction is the sole means of determining the chemical composition of surface films, even the existence of which may be difficult to ascertain by any other method.

The general tendency of electro-deposits is to follow the crystal-size and structure of the basis metal, and this seems to be a determining factor in adhesion. While no clear evidence of alloy formation at the basis metal-deposit interface has so far been observed in electro-deposition, alloys are formed when one metal is chemically displaced by another. Examples are given of cases in which microscopic examination of metallic structures may lead to wrong inferences.

In surface films of the normal paraffins, the long-chain molecules stand vertically on the surface, but in the corresponding end-substituted compounds the molecules lean over to an extent which depends upon the size of the substituent group. The role of such films in lubrication is discussed.

Dr R. Beeching said that the more widespread employment of electron-diffraction for the attacking of surface problems had been advocated vigorously for some time, and there had, in his opinion, been over-emphasis of the powers and possibilities of the method, without due regard to the difficulties which may arise. He felt that it must be for the ultimate good of the subject to present these difficulties in their true light.

Quite a number of surfaces give no pattern of any kind. This may be due to a variety of reasons, but it is certainly a severe limitation to the method in some cases. Charging up of the specimen in the electron beam also may constitute such a serious difficulty as sometimes to render the method useless.

In short, invaluable as the method undoubtedly is in some instances, its application is limited in a way which has frequently not been made clear.

A difficulty of another sort—one which has hampered the development of the subject, rather than a difficulty in its application—has been the lack of a satisfactory theory, independent of X-ray diffraction theory. There are a number of points about single crystal patterns which cannot as yet be explained by any existing theory.

Dr A. G. QUARRELL said that since, in general, the surface layers of a specimen cannot be removed with the certainty that no structural or chemical change has been caused by the stripping process, it follows that the reflection method must be used when electron-diffraction is applied to the study of surface structure. Unfortunately, the reflection method is subject to certain limitations from which the transmission method is free. Thus, with polycrystalline reflection specimens a characteristic pattern will result only if crystallites sufficiently thin to transmit electrons project above the general surface. In transmission, the distance from the diffracting crystal to the photographic plate is the same for all crystals, but in the grazing-incidence method there may be a difference in this length equal to the diameter of the specimen. It is for this reason that the diffractions obtained by reflection are never as sharp as those obtained by transmission from a comparable specimen.

Further, in electron-diffraction generally, the diffraction rings of small radius are most useful in determining the structure of the specimen. In the case of $\alpha$-$Fe_2O_3$ and $Fe_3O_4$ the diffractions almost coincide, with the exception of the first two or three rings, and it is essential to use up-to-date technique in order to obtain these rings clear of background.

In view of the importance of determining the spacings of the innermost rings with maximum accuracy, the split-shutter method had been developed for reflection specimens. The reference specimen consists of lead sulphide, freshly precipitated and washed, applied by means of a camel-hair brush to one half of the specimen, and is thus co-planar with the specimen under examination. Lead sulphide has the advantage that it possesses a simple structure and gives a number of rings in the region where accuracy is most desirable.

Since the grazing-incidence method in effect gives the average structure of the strip of specimen traversed by the beam, and since it is not applicable directly to rough surfaces, the possibility of back-reflection of electrons had been examined, the electron beam being normal to the specimen surface. In this way spot patterns had been obtained from a deeply etched iron surface which could not be examined by the reflection method, and a particularly clear spot pattern had been obtained from a quartz single crystal. It was too early to discuss the mechanism of diffraction involved, but it seemed clear that with further work this should develop into a useful method, applicable where the earlier methods of electron-diffraction fail.

Dr W. Cochrane. I should like to mention an aspect of the subject which has recently developed—the determination of submicroscopic surface form. The explanation of the irrational interferences observed by various experimenters has always been a perplexing problem, but von Laue's theoretical investigations seem to show that they arise from the nature of the bounding surfaces of the crystals. It is well known that the patterns obtained by electron-diffraction can be interpreted in terms of the hypothetical reciprocal lattice, in which each point represents a plane of the real lattice. A sphere of radius $1/\lambda$ is drawn, where $\lambda$ is the wave-length of the electrons, and the points of the reciprocal lattice intersected by the sphere appear as spots in the photograph. In general only a few points are intersected. But von Laue has established a rule which says that, if $P$ is a plane bounding surface of the crystal, then a short spike, pointing in a direction normal to $P$, must be attached to each point of the reciprocal lattice. The sphere then intersects many of these spikes, giving corresponding spots in the pattern. Conversely, we can work back from a given pattern and find the directions of the spikes and thus deduce the bounding surfaces. This, however, will be possible only when the surface of the specimen is fairly uniform and reasonably simple in its topography, for if all sorts of bounding faces are present the effect will be lost.

Some photographs which illustrate this method appeared in the *Proceedings of the Physical Society*, **48** (1936), in the plate facing page 734. Figures 8 and 9 of that plate show the patterns obtained from an electrolytically deposited cobalt surface. The pairs of spots in figure 8 and the groups of fours in figure 9 show that there are here four spikes, although in figure 8 the crystal is set so that the sphere cuts only two of the spikes. It is easily deduced that the surface consists of small projecting lumps bounded by octahedral faces. A view looking down on such a lump is shown in figure 5 of the same paper. Figure 7 of the plate is from an electrolytically deposited nickel surface and here there are only pairs of spots. It is deduced that the surface is composed of rather long corrugations or ridges of triangular cross section. One of these corrugations seen end-on is shown at the bottom of figure 1 of that paper. When these photographs were published, von Laue's theory was not available and I deduced indirectly, from the presence of twin spots, that crystal-growth was taking place very regularly on octahedral faces during the deposition of the metal. This gives a simple physical picture of how the projections are built up. It should be noted, however, that Kirchner has found cases where twinning on octahedral faces occurred but yet the boundaries forming the free surface were cube faces.

By a more detailed and quantitative examination of the patterns von Laue is able also to deduce the size of the projections on the surface, and he finds that their dimensions are about $6 \times 10^{-7}$ cm. so that their shape could certainly not be distinguished with a microscope.

Dr W. H. J. **Vernon**, commenting on Prof. Finch's statement that an amorphous surface film confers as a rule greater protection than a crystalline one, remarked that the available evidence (Preston and Bircumshaw's results for aluminium and Prof. Finch's for passivated iron) is scanty so far as metals are concerned; it is, however, supported by recent work on zinc at the Chemical Research Laboratory, done in collaboration with Dr Shearer of the National Physical Laboratory. The physical condition of the initial metal surface is an important factor, but he deprecated the very common misinterpretation of the findings of Bowden and Ridler, to the effect that the melting-point of the metal is necessarily reached, even momentarily, in polishing; actually these workers concluded that the temperature reached is a linear function of load and speed, with the melting-point of the metal as the limiting temperature. He supported Dr Beeching's caution as to the limitations of electron-diffraction methods, notably in respect of the difficulties associated with negative results, and he emphasized the essentially qualitative nature of the information, notwithstanding its great importance and value in specific cases. He urged the closest possible co-operation between electron-diffractionists and those who are applying other methods to the study of these thin films.

Dr J. A. **Darbyshire**. I would like to describe some results that we have obtained whilst examining oxide-coated cathodes by the electron-diffraction method in the Research Laboratories of Ferranti, Ltd. Oxide cathodes are prepared by spraying a paste, consisting of finely divided mixed crystals of barium and strontium carbonates, or a mechanical mixture of these carbonates, on to a nickel sheath which forms the core of the cathode. This nickel sheath is provided with an internal heating element, and after being sealed into the electron tube the cathode is activated by heat treatment in vacuo. The carbonates decompose into oxides at approximately 800° c., and after they have been flashed for 4 minutes at 1000° c. current is drawn from the cathode at its normal working temperature (about 750° c.) until the thermionic emission reaches a high value.

Various theories have been put forward as to the physical and chemical state of the cathode surface when the emission is a maximum. The theory most generally accepted postulates a monatomic layer of barium resting on the mixed oxides of barium and strontium(1). Electrons are diffracted only by the first 20 atomic layers or so, and it was hoped that they would give some evidence for this surface layer of barium or, in any case, give some interesting information about the outer surface of the cathode(2).

The diffraction camera was fitted up so that the cathode could be activated inside the camera and getter pellets could be fired off at suitable stages of the activation. The diffraction photographs from sprayed cathodes in the unactivated state were very poor, and on microscopic examination these surfaces were seen to be quite rough. Much smoother surfaces could be obtained by pouring the mixed carbonate suspension on to the cathode surface and allowing it to dry off in a horizontal position. The photographs of the unactivated carbonates were never very good, but they corresponded to the x-ray patterns and the known structures for the carbonates, figure 1. The activated cathodes gave much better patterns, figure 2, and well-activated healthy cathodes gave patterns of strontium oxide without any evidence of barium oxide or a solid solution of these oxides. This result corresponds to that obtained by Gaertner. Photographs of cathodes prepared from barium carbonate alone gave barium oxide, and those from strontium carbonate alone gave strontium oxide. The emission from the strontium-carbonate cathodes was very much less than that from the mixed carbonates, although the electron-diffraction patterns indicated strontium oxide in each case. There was no evidence for difference in lattice constant, but the 440 reflection from the strontium oxide of the normal

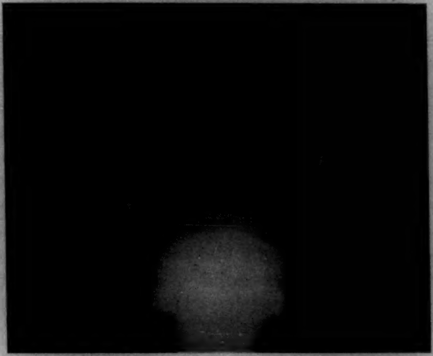

**Figure 1.**

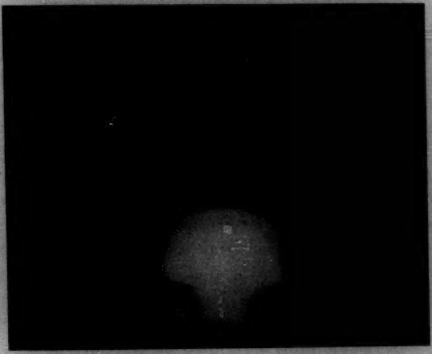

**Figure 2.**

mixed-carbonate cathode was much stronger than the 440 reflection from ordinary strontium oxide.

The lattice constant of barium is 5·015 A. and that of strontium oxide is 5·15 A., and so it is possible that there may be a partial monatomic layer of barium adsorbed to the strontium-oxide lattice, most probably by being bound to the oxygen atoms of the strontium-oxide structure. This may be revealed by a careful comparison of the relative intensity of the lines from the activated strontium-carbonate cathode with those of the normal mixed-carbonate cathode.

Dr H. **Wilman** said that in his opinion the experimental technique of electron-diffraction is not very difficult, as Dr Beeching holds it to be. Like all techniques, however, it must be acquired by experience, and all necessary and reasonable care must be taken to avoid adventitious impurities and to study the material under investigation by means of specimens in the form most suited to give clear electron-diffraction patterns.

The use of electron-diffraction to estimate the mean size and shape of crystals can be illustrated by reference to the case of colloidal graphite particles. Reflection patterns yielded by a film adsorbed from the colloidal solution show that the colloidal particles have the normal crystalline structure of graphite. From the large breadths of the diffractions due to planes normal, or nearly normal, to the *c*-axis, the mean crystal thickness in the *c*-axis direction is estimated to be only about 10 carbon-atom layers, whereas planes nearly parallel to the *c*-axis yield sharp diffractions; hence the crystals must measure at least 200 A. in directions normal to the *c*-axis. Some of the larger of these flake-like crystals can, indeed, be seen in the ultra-microscope, and some even in the ordinary microscope. Patterns obtained by transmission of electrons through the colloidal graphite dried out on a thin collodion film confirm the flake-like form of the crystals in a similar way and show, like the reflection patterns, that the flakes orient themselves parallel to the substrate surface. The appearance of certain abnormal diffraction rings also shows the extreme thinness of a large proportion of the flakes.

It is interesting to note that the X-ray patterns obtained from colloidal graphite are so diffuse as to suggest, entirely erroneously, that the material consists of amorphous carbon. The very diffuse X-ray diffractions yielded by some colloidal clays also are apt to lead to a false view of the particle-form, where the sharp and strong electron-diffraction patterns clearly establish their crystalline flake-like nature.

Dr R. O. **Jenkins**. Oxide films on molten metals have usually in the past been investigated by removing them from the metal and using transmission electron-diffraction. These films generally show only partial orientation. Examination of the surface of molten metals in vacuo gave reflection patterns consisting of rings and a faint streak, the first from a relatively thick film of oxide and the latter from a very thin film between cracks in the thick film. After the thick film had been swept away by means of a razor blade in vacuo the streak and spot pattern was obtained very strongly. The pattern showed that the metal surface was covered by a very thin electron-optically flat film of oxide which was always oriented so that the plane on the metal surface was the one with the most metal atoms per unit area. From these patterns it was possible to identify the oxide and calculate its inner potential.

The film on tin just above its melting-point is a tetragonal lattice of SnO resting on its (001) plane, and at a higher temperature is a tetragonal body-centred lattice of $SnO_2$ resting on its (101) plane. Lead is covered with a film of yellow PbO known as litharge in an orthorhombic lattice resting on its (001) plane, and zinc with a film of ZnO in a hexagonal lattice also resting on its (001) plane. The pattern obtained from bismuth was almost identical in form with that from zinc and indicated a hexagonal-lattice oxide, in which $a=4{\cdot}0$ A. and $c/a=1{\cdot}57$, resting on its (001) plane. This has never been studied with X rays but must almost certainly be $Bi_2O_3$ isomorphic with the trivalent rare-earth oxides.

When the metal was allowed to cool slowly and form large single crystals it was found that the small oxide crystals had turned under the action of the forces from the underlying metal lattice so that they formed a single-crystal oxide film. This gave a typical single-crystal pattern of spots and Kikuchi lines and showed that the oxide film was still resting on the same plane as before and that movement was only round an axis perpendicular to the metal surface.

THE PRESIDENTS OF THE TWO SOCIETIES, Prof. FERGUSON and Prof. DONNAN, expressed the pleasure and satisfaction which this, the first joint discussion to be held, had given to the members of the two societies.

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(1) BECKER. *Phys. Rev.* **34**, 1323 (1929).
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# OBITUARY NOTICES

## DAVID BAXANDALL, A.R.C.S., F.R.A.S.

MR DAVID BAXANDALL, who in April 1934 retired from the position of Deputy Keeper at the Science Museum, passed away on 10 January at the age of 63, after a short illness.

He was born on 9 April 1874, near Keighley in Yorkshire, and received his early education at the Keighley Institute. In 1891 he won a Royal Scholarship with which he proceeded to the Royal College of Science at South Kensington, where his brothers had already preceded him. Here he studied mechanics under Henrici, obtaining his first-class associateship in 1894. He remained at the college as a demonstrator and assisted Lockyer until 1898, when he entered the Science Museum. It was there that he developed a keen interest in early scientific instruments and became a leading authority on the subject, and it is largely due to his unremitting efforts that the collection of scientific instruments and apparatus at the Science Museum is so rich in early and valuable objects.

He was a member of several learned societies, at the meetings of which, however, owing to his natural reticence, he could seldom be persuaded to speak. He contributed a number of papers to the Optical Society, including one on the circular dividing engine of Edward Troughton, and another in which he described two replicas of Galileo telescopes made by Cipriani in Florence.

In May 1923 he presented another paper before the Society on early telescopes. The invention of the station pointer had until recent times been attributed to Nicholson, but Baxandall refuted this claim, and in a concise paper in the *Empire Survey Review* produced evidence that the real inventor was Joseph Huddart, F.R.S.

Mr Baxandall was closely associated for many years with Mr Thomas H. Court, with whom he contributed two articles to the *Proceedings of the Optical Convention* of 1926; one on a "Telescope made by Christopher Cock in 1673", and the other on "Early Optical and other Scientific Instruments as documents of historical value". In the latter paper it was stated that the authors had, during the preceding 20 years, seen and examined many thousands of early microscopes, telescopes and other optical instruments made by English makers. It was an appeal for the preservation of early instruments with reference to some systematic attempts which had been made to acquire, classify and preserve collections.

Mr Baxandall was elected Fellow of the Royal Astronomical Society in 1912, Member of the British Astronomical Association in 1923, and Member of the Optical Society in 1916, while he was also a Member of the Optical Conventions of 1912 and 1926.

He was modest, thorough, and entirely free from pretence, and those with whom he came into contact will remember with admiration the honesty of his nature, and his conscientious devotion to his work.

At the time of his retirement he had been Deputy Keeper in charge of the Science Division of the Science Museum for over 12 years.

H. S.

## MR A. M. CODD

MR ARTHUR MORTIMER CODD was born in 1880 and educated at Harrow. He later entered as a student for the electrical engineering course at the Central Technical (now the City and Guilds) College, South Kensington. After completing this course he joined Messrs Johnson and Phillips, Charlton, as a premium pupil, passing through the various shops and drawing offices, and on completion of his training he specialized in inductance and high-frequency apparatus.

In 1906 he began business on his own account as the High Tension Company, renting a small shop at the works of Johnson and Phillips, where he manufactured ignition and inductance coils for medical work. The business was later transferred to Hanway Street, Oxford Street, and finally to Belvedere Road, Westminster, where he was established for many years as a manufacturer of ignition coils, the Echo motor horn, and car-lighting generators. His apparatus for the latter purpose was among the first of its type to be used, and was very successful in the use of permanent magnets for the field and car-lighting dynamos. Mr Codd was one of the pioneers of car lighting by electricity, for his first model, the Mira Magnetolite, was produced as long ago as 1909. The Mira high-tension coils are still in use for naval purposes.

A much needed text-book, *Electrical Ignition for Internal Combustion Engines*, was written by him in 1911, and was the standard work of reference for automobile drivers at that period. The book was followed by *Dynamo Lighting for Motor Cars*, published in 1913. Throughout the War he was engaged in the production of many types of high-tension coils for the services.

Some 14 years ago Mr Codd retired from manufacturing and devoted himself to consulting work. He became an active contributor to the technical journals, the chief subject of his articles being primary batteries and high-tension apparatus.

He was for many years a member of the Physical and Röntgen Societies and a regular exhibitor at the Physical Society's annual exhibitions. His novel and beautiful Wimshurst machine, for supplying current to x-ray tubes and the like, will be remembered by visitors and readers of this journal. The painstaking and valuable work done by pioneers of Mr Codd's type is too easily forgotten, and his death at the height of his activities will be deeply regretted. He leaves a widow and a son and daughter.

## C. W. S. CRAWLEY

MR CRAWLEY passed away on 9 November, 1937, aged 79. Although after the War he retired to Charlbury in Oxfordshire, before that he was well known in electrical and motoring circles. Educated at Bonn, he afterwards attended the school of engineering in Hanover Square. He then became an original partner in the firm of Nalder, Crawley and Soames, whose electrical measuring instruments became so well known under the title of N.C.S. in the early days of electric lighting. Mr Crawley was the technical partner in this firm and devoted much time to the construction of standard instruments, and he improved the methods of calibrating them. Later, Mr Crawley's firm decided to give up the more scientific side of their business in favour of the more commercial, and Mr Crawley left them. From this time, having a modest competence, he rather thought of helping others and advancing the cause of science than of making money for himself, and of this work the following examples will be given.

The Board of Trade Laboratory had, at that time, not long been set up with the view of providing standards for the benefit of the rising electrical industry. While practising as a consulting engineer after his retirement from Nalder Bros. and Co. (now Nalder Bros. and Thompson) he spent much of his time in the Board of Trade Electrical Standards Laboratory, voluntarily assisting Mr Rennie, but practically unknown to anyone in the Board of Trade, except Mr Trotter, Mr Rennie and his assistant. He worked chiefly in the resistance room, making highly accurate comparisons of the standards, some of which had been made by Nalder Bros. and Co. in 1892 for the Electrical Standards Committee of the British Association. The Board of Trade standards had been verified by that committee and deposited under the Order in Council of 1894. From a scientific point of view the work was rather disheartening, for it was known that some of the British Association coils had changed owing to acidity in the paraffin wax used for insulation, and other causes, and the redetermination in absolute value was not within the province of the Board of Trade. Major Cardew and Mr Rennie had used them for comparison with resistances sent to the laboratory for certificates, but the methods and apparatus employed were but little better than the early practice of the British Association Committee.

Mr Crawley presented a master coil to the Board of Trade in 1900, the temperature coefficient of which had been measured with extreme care. He devised the rearrangement of the resistance room, constructed the oil tank, thermoelectric and other temperature apparatus, and thermostats. He recognized that the weak point in most measurements of resistance had been the determination of temperature. At his suggestion Dr Guillaume was asked to procure a thermometer of the highest precision, and M. Tonnelot made one, which was brought from Paris by hand. It was graduated to tenths of a degree, but by optical and thermoelectric methods readings were booked to one thousandth of a degree. Mr Crawley considered that high precision might be attained by measurement of temperature rather than by means of the low temperature coefficients of alloys. He gave scrupulous care to corrections. Among those provided for the thermometer was one for atmospheric pressure, and half seriously he put a scale of thousandths of a degree of temperature on an aneroid barometer. He elaborated the Carey Foster slide-wire bridge until differences of one-fifth of a microhm could be read: a modification of the Nalder pattern was used, allowing the two coils to be compared while they were side by side in the oil bath. He devised build-up boxes for passing from 0 Ω. to 10,000 Ω., only two mercury cups being used at a time; these and the differential galvanometer and thermostat were designed by him and made while he was with Nalder Bros. and Co.

At the International Conference on Electrical Units and Standards in 1908 Mr Crawley was one of the secretaries, and his fluent knowledge of French and German was a great help to the delegates.

He also knew personally many of the pioneers of the motor-car industry; he helped with trials and in the automobile clubs' survey of the country, which resulted in the first signs for warning and direction being erected.

At this time Mr Crawley lived at Putney, where he had a very complete workshop for electrical and mechanical work, and devoted much time to unobtrusively helping other workers, amongst whom were the late Dr George Forbes, then engaged on designing range-finders.*

Crawley was a strong Unionist, and before the War, when the affairs of Ireland began to reach a critical stage, he joined a Corps of Volunteers who were ready to proceed to Ulster if matters came to a head. They used to drill in the evening in Holland Park, and Crawley was in the habit of going about with a short rifle, in the barrel of which the handle of an old golf club was fixed, in his golf bag.

Shortly after the War had begun, declaring that his age was under 40 (although he was really nearly 60) he succeeded in enlisting in the anti-aircraft force, where his mathematical and mechanical knowledge was useful, and for a long time he was in charge of a battery of anti-aircraft guns on the east coast.

After the War, he and his sister removed to Charlbury in Oxfordshire, where he again set up his laboratory. He had an idea that a great deal could be learnt by study of the minor and more rapid fluctuations of the barometer, and spent a good deal of time in devising a special registering barometer for this purpose.

His recreations were golf and fishing, and he was sufficiently expert to build his own flyfishing rod with its tackle.

With his death passed one of the few remaining men connected with the rise of the modern electrical industry from its early stages.

Mr Crawley became a Life Fellow of the Physical Society in 1895.

G. L. A.

## CHARLES-EDOUARD GUILLAUME, 1861–1938

FLEURIER, a small village in the Swiss Jura, ten miles from the French frontier, saw the birth of Charles-Edouard Guillaume. Except for a spell of exile during the French revolution, his family had been living there for three centuries. There his childhood was spent, in the midst of men whose living was derived from agriculture in summer and the making of clocks and watches during the snow season. His father, himself a watchmaker, was his first master. He was fifteen when he first left Fleurier; by 1878 he was a student, and a brilliant one, at the Zürich Polytechnicum. After the completion of this course he became an artillery officer, interesting himself in ballistics. But this did not last.

By 1883 we find him, a lad of 22, entering the Bureau International des Poids et Mesures, an organization which had begun work a few years before. O. J. Broch was then Director. J. René Benoît was one of the most distinguished workers, and under his influence the foundations of modern metrology were being built with beautiful honesty and enthusiasm. The following anecdote is not irrelevant, since it gives an indication of the atmosphere which was to surround Guillaume for the rest of his life. In 1889 intercomparisons between the platinum-irridium copies of the International metre were being carried on, and Benoît had been examining the accuracy of a few hundred measurements by the mean-squares method. The concordance was most gratifying, except in a single instance in which an inexplicable discrepancy of $1\,\mu$. was to be found. René Benoît came to luncheon in a state of absolute dejection, and sat munching slowly, repeating, at intervals, with the mechanical precision of a clock: "One *whole* micron!...one *whole* micron!..."

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* He also helped the writer in his work on dielectrics for several years.

This delightful man chose Guillaume for his assistant. From 1883 to 1889, studies in thermometry, the determination of the thermal expansions of the standards of length were their chief concerns. In 1889, Guillaume's *Traité Pratique de la Thermométrie de Précision* was published: every metrologist has read this fundamental work. Off and on, Guillaume was to be concerned with thermometric and metrological work all his life, but in 1895, there happened the incident which started him on a series of researches whose initial purpose was purely metrological, but whose consequences became of outstanding importance in metallurgy. The Bureau had long been interested in non-oxidizing alloys, as cheap substitutes for platinum in the making of standards. The Imphy steel works sent Benoît a sample of their $NC_4$ alloy—a ferronickel containing 24 per cent of nickel; it was found that the thermal expansion of this alloy was actually greater than that of its constituent metals. Some time afterwards, in 1896, Guillaume measured the thermal expansion of an alloy containing 30 per cent of nickel and found it to be half that of pure iron. That day began his patient investigation of the physical properties of ferronickels, which, with the help of the Imphy steel works and his co-workers, chief amongst whom was his pupil Chevenard, progressed for forty years and is indeed still progressing, though its originator is dead. It led successively to the discovery of invar, elinvar and baros; later on, a minute study of the stability conditions of these alloys produced stable invar, universally known and used now; modified elinvar followed as late as 1919 and is now used the world over in the making of spiral watches. Those long studies in physical chemistry are dotted with applications: Guillaume it was who built non-dilatable standards of length, designed the first invar wires for the measurement of geodetic bases, and invented the integral balance for the regularization of watches and chronometers. It is because Guillaume has lived and worked that the Short clock No. 44, with an invar pendulum, of the Paris Observatory had in 1935 a maximum variation of 0·00017 sec. as compared with the mean monthly values.

Since the death of René Benoît in 1915, Guillaume had been director of the Bureau International des Poids et Mesures, and up to his retirement in 1936 much of his time was occupied by the government of this small nation. Under his administration, the Bureau remained flourishing and its traditions were kept. The life of Charles-Edouard Guillaume, both public and private, is inseparable from the Pavilion de Breteuil, its laboratory and its garden: many distinguished men from many nations visited him there, and found him a simple, courteous, charming and wise man. He was patient and thorough, but not plodding; his metallurgical work often shows brilliant intuition. He was modest, and honours were heaped upon him unsought. A Nobel prize was awarded him in 1920; the Physical Society, before which he delivered the Guthrie lecture, presented him with the Duddell medal in 1928. It is not possible to give here a list of the honours and decorations which he received from everywhere. He loathed advertisement, was a staunch friend and foe, and a man who loved work well done. He used to say that "nothing in Science has so many definitive consequences as the gain of one place in decimals". The incomprehension of the fundamental importance of metrology displayed by some physicists could throw him into fits of rage; on those occasions, he could be caustic. One of our more noisy physicists having one day described metrologists as plodding and short-sighted fellows with few ideas, Guillaume answered that the difference between a metrologist and such a physicist as his interlocutor was the same as between a plough-horse and a racehorse: "When the race is ended, what remains? Dust, a little noise; some money has changed hands. Where the plough-horse has pulled, corn may ripen some day." He was right.

G. A. B.

## EMERITUS PROFESSOR WILLIAM STROUD, M.A. (OXON.), D.SC. (LONDON), HON.D.SC. (LEEDS), F.INST.P.

PROF. STROUD, who was chairman of Messrs Barr and Stroud, Limited, died at Torquay on 27 May 1938 at the age of 78.

As a schoolboy in Bristol he soon gave evidence of outstanding mental ability. He obtained an entrance scholarship to University College, Bristol, in October 1878 at the age of eighteen, and eight months later a Gilchrist Scholarship to Owens College, Manchester. A Brakenbury Science Scholarship took him to Balliol College at the early age of twenty. In the London University examinations and at Oxford his record of first class honours was outstanding. Sir Henry Roscoe described his academic career as a most distinguished one not only in what are called the schools but in the far higher regions of scientific investigation, especially in the subject of electricity.

Professor Jowett, Master of Balliol in 1885, referred to him as a young man of great force of mind and character who might become an original discoverer, and Prof. Kohlrausch, in whose Laboratory at Würtzburg he spent one Semester in 1884, refers to his skill and vision in practical work. A portion of the vacation of 1883 was spent in the Physical Laboratory at Heidelberg under the direction of Prof. Quincke.

Soon after his return to England the Yorkshire College in June 1885 lost by resignation Sir Edward Thorpe and Sir Arthur Rücker. Notwithstanding his lack of professorial experience and his youth, for he was only twenty-five, William Stroud was appointed successor to Rücker as Cavendish Professor of Physics. Two months thereafter, in August 1885, he met there for the first time the professor of engineering, and thus commenced a friendship and association that was only terminated 46 years later by the death of Prof. Archibald Barr.

The first results of their collaboration were a lecture-room projection lantern, and apparatus for the ready production of lantern slides from any book illustrations. Both devices are still extensively used. About the beginning of 1888 they decided to collaborate in research work. In the words of Dr Stroud "the times were very curious—science was in a state of frozen immobility. Almost the only Physics then known was the physics of brass and glass. Mechanically propelled vehicles had a maximum speed of only 3 miles per hour and were compelled by law to be preceded by a man with a red flag. Aviation had been proved impossible by Lord Kelvin. Motor cars were unknown. In a world of pure science X-rays were unknown; radium had not been discovered. Nothing was known about argon and the rare gases in the atmosphere. Electrons, isotopes, quanta and heavy hydrogen had not been heard of, and wireless was in the future. The redeeming feature was that income tax was only 6*d*. in the £ and death duties had not been imposed."

As a first investigation they decided to determine, if possible, with still greater accuracy the mechanical equivalent of heat, but a few months later this intention was abandoned when Prof. Barr saw a War Office advertisement in *Engineering* for an infantry range-finder, preferably single-observer, having an accuracy of 4 per cent at 1000 yards. Although neither knew anything about the subject, they decided to enter for the competition and quickly produced a first rough instrument which at least made them realize how difficult the problem was. In less than a month they applied for a patent for their first range-finder, specification no. 9520/1888. The designs were approved by the War Office but no financial assistance was afforded the inventors. It was a condition of the trials that the instruments should be delivered by 31 December 1888, which left very little time for detailed design and construction.

In view of the difficulty of obtaining satisfactory end prisms, silvered reflectors were employed with disastrous results. The morning of the trials was cloudy. After luncheon, however, the sun emerged and its rays falling on the mirrors distorted them so seriously that the readings were affected. In a few months they were informed by the War Office that the instrument was not suitable for adoption in Her Majesty's Service. The Admiralty, however, regarded the results as promising, and in 1891 invited the inventors to submit an instrument for competitive trial in H.M.S. *Arethusa*.

Watkin's two-observer instrument was rejected as unsuitable owing to the restriction of its effective arc of operation, and the single-observer range-finder of Mallock, the Astronomer Royal, proved less accurate than the Barr and Stroud instrument for which an order for six was soon placed.

When Prof. Barr in 1890 was appointed to the Regius Chair of Engineering at Glasgow it was feared that the collaboration would end, but fortunately, as no restrictions were imposed upon the occupant of the Chair, Prof. Barr was able to carry on the actual work of construction more effectively than he could have done in Leeds. The range-finder was soon adopted by the armies and navies of the principal world powers, and as the small laboratory and workshop, conveniently established in Ashton Lane between Glasgow University and the house of Dr Barr, could not be satisfactorily extended, the first portion of the present factory was erected at Anniesland. This factory to-day produces not only a great variety of range-finders, submarine periscopes, sights of many kinds, and fire-control gear, but also its own optical glass, of which unusually large discs and blocks are required, and anti-vibration rubber for the naval mountings. Owing to the rapid development of the establishment, satisfactory collaboration by correspondence became difficult, and the presence of Dr Stroud in Glasgow became increasingly necessary. In 1909 he accordingly tendered his resignation of the Cavendish Chair, not only on account of the business, but also in the interests of the university in Leeds, the work of which should, he considered, receive the undivided attention of a younger man prepared to advance with enthusiasm the new physical conceptions of radiant energy. Some time before the death of Dr Barr he found it necessary to seek retirement in the more kindly climate of the south, but later after some persuasion he accepted the vacant chairmanship and found great pleasure in his visits to the scene of so many years' successful scientific and practical activity.

The genial and kindly personality of Prof. Stroud, his ability as a student, his efficiency as a teacher, his unsurpassed capacity for the application of science to industry, and his sympathetic regard for his many employees, are characteristics that have endeared his memory to all who throughout his life were privileged to be associated with him.

J. W. F.

## THOMAS SMITHIES TAYLOR

THOMAS SMITHIES TAYLOR laid the foundations of Taylor, Taylor and Hobson, Ltd., 52 years ago, and took an active interest in the business up to the time of his death.

He was first apprenticed to Howard's of Bedford, and later to R. and J. Beck, of London. This apprenticeship involved a ceremony at the Goldsmiths' Hall and conferred the status of Freeman of the City of London upon him. In 1886 he left Messrs Beck and started business for himself in Leicester as a maker of lenses, with a capital of £300. Within a few months he was joined by his brother William, who died last year, and by H. W. Hobson. It was largely owing to his initiative and enthusiasm that when, in 1893, H. D. Taylor, of Cooke and Sons, York, invented the Cooke photographic lens, the manufacture was licensed to Taylor, Taylor and Hobson, Ltd.

He was a member of the Territorial Association, and served during the War in the Royal Army Service Corps, and later he was, for a time, Estate Manager at Bedales School, Hampshire.

In all business matters he was keen and orderly, and in personal contacts he was helpful, kindly and generous to a fault; to all who knew him intimately a fine friend.

A. W.

# REVIEWS OF BOOKS

## *Statistical Physics*, by L. LANDAU and E. LIFSHITZ. Translated by D. SCHOENBERG. Pp. viii + 234. (Oxford: Clarendon Press.) 20*s*. net.

The plan of this book is, roughly speaking, to develop thermodynamics from the statistical point of view introduced by Gibbs. His work is very difficult to understand, partly because he is in the habit of embarking on investigations without stating beforehand where they are to lead, and partly because, when he reaches an important result, he does not point the fact out. Thus a mere restatement of his work would be valuable, but the volume before us contains more than this. It develops its subject in an order dictated by logic and not by our experimental familiarity with various concepts, so that energy and entropy appear before temperature, which presents itself as a mere differential coefficient, proved to be constant throughout a closed system which is in equilibrium.

Systems requiring quantum laws are excluded from the scope of the book, but many problems of which connected accounts cannot easily be found elsewhere are included, such as the theory of the Van der Waals gas, and the thermodynamics of solutions which are not very dilute and of ordered solid solutions.

Attention may be directed to the excellent discussion of the difficulties connected with the principle of increase of entropy and the associated difficulty that in a reversible system we "ought" to be able to reverse the sign of the time, and so obtain changes which obey the mechanical laws and yet, since they retrace the steps by which equilibrium was approached, correspond to a decrease of entropy.

Although it shows a few signs of being a translation from a foreign language, the book is well written, and can be recommended to all students of theoretical physics.

J. H. A.

## *Physik*, by Ing.-Dr P. WESSEL, edited by Dr V. RIEDERER VON PAAR. Pp. xii + 550. (Munich: Ernst Reinhardt.) RM. 4.90.

This German textbook is intended to cover the requirements of students in their first semester at the University, and corresponds approximately to our text books for intermediate students. Its divisions are much the same as those in an English text book of similar standard. The first part deals with mechanics, sound, heat and light, the second with magnetism and electricity and atomic physics, whilst the third is a short résumé of the whole text, together with a collection of formulae and 29 tables. It is interesting to note that in the paragraph on specific heat the calorie is first defined; specific heat then follows as the number of calories required to raise one gramme of a substance 1° C. This we believe to be much preferable to the more usual method adopted in English text books of defining the specific heat as a ratio. Strictly speaking *all* measurements are ratios—ratios of the quantity to some given standard unit; and yet it is usually only the definition of specific heat that is made to include also the definition of the standard unit. (Specific gravity, fortunately, is being gradually dropped in favour of the more rational measurement of density.)

The book should be of help to students seeking a knowledge of technical German. It is written in fairly simple German and is clearly printed, so that with an elementary knowledge of the language and some knowledge of physics a student should be able to follow it quite readily.

H. R. L.

*Grimsehls Lehrbuch der Physik*, revised by R. TOMASCHEK. Vol. II, Pt. I. Electromagnetic Field and Optics. 8th ed. Pp. x+866. (Leipzig, B. G. Teubner, 1938.) RM. 26.

No efforts have been spared to keep Grimsehl's text book up to date, both by the inclusion of new matter and by the rewriting of parts of the work where experience has shown that the treatment can be improved. A seven-page article on electron optics is new in this edition, and amplified accounts are given of several other topics. In the revised treatment of electromagnetism, greater stress is laid on Maxwell's equations, but here there is still room for improvement. Somewhere in this book the complete system of Maxwell's equations should be put before the student and he should be shown how these equations lead in a perfectly general way to the differential equation of wave motion.

Taken as a whole, this work can only be described as an excellent text book, excellently produced. Prof. Tomaschek is to be congratulated on maintaining its very high standard.

W. S. S.

*Die Physik des 20. Jahrhunderts*, by P. JORDAN. 2nd ed. Pp. x+159. (Braunschweig, Vieweg, 1938.) RM. 4.80.

The author sets himself the difficult task of drawing in broad outline a complete picture of modern physics. Details of experiments and mathematical formulations of theory are excluded, and attention is concentrated on the underlying ideas which have given direction to the efforts of physicists from the time of Galileo to the present day. The result is an interesting account of the notions of force and motion, action at a distance and through a medium, ether and relativity, the reality of atoms, the wave-particle dualism, quantum theory, causality and so on. Great stress is laid on the "liquidation of materialism" and the triumph of scientific "positivism", a change of view which obviously has an almost mystical significance for the author. This is particularly apparent in the discussion of the bearing of the recent developments in physics on other aspects of the world picture (Weltanschauung) such as religion and the theory of knowledge. The book would be improved by the omission of all such excursions from the domain of physics proper. Another improvement for many readers would be the omission of the remarks on p. 37 which belittle Einstein's achievements in the theory of relativity.

It is something of a feat to have written this book without introducing a single diagram or mathematical symbol, but it is doubtful whether the wider circle of readers whom the author has in mind will find it very easy reading.

W. S. S.

*Kontinuierliche Spektren*, by W. FINKELNBURG. Pp. xi+368. (Berlin: Julius Springer, 1938.) RM. 33; bound RM. 34.80.

It is fitting that the well-known series of monographs, *Struktur und Eigenschaften der Materie*, which already includes several important works on atomic and molecular spectra such as those by Back and Landé, Hund, Grotrian and Sponer, should now include a comprehensive account of present knowledge of continuous spectra. To physicists, astronomers and chemists this will be especially welcome, since nearly all the available books on either line spectra or band spectra neglect continuous spectra almost entirely. We have already had from the present author very useful monographs on continua in the *Physikalische Zeitschrift*, *Die Physik*, etc., but never have we had in a single volume such a wealth of information on the subject. Every type of continuum is adequately dealt with, and the descriptions are invariably accompanied by the relevant parts of the theory of atomic spectra, both X-ray and optical, and molecular spectra.

After a useful introductory chapter, there are five chapters on atomic electronic continua—the general theory, absorption-limit continua and photo-ionization, electron recombination and series-limit continua in emission, free-electron continuous radiation and absorption, perturbations. Then follow five chapters on molecular continua—general considerations, theory, typical cases, continua of special diatomic molecules, continua of polyatomic molecules. The next three chapters deal with line-breadths, continua of liquids, solutions and crystals, and temperature and black-body radiations. In the remaining two chapters, dealing entirely with the experimental and technical aspects of the subject, we have a review of observed gas continua (absorption and emission) arranged in chemical groups, and an account of the methods of production of emission continua. The text contains 25 short tables of numerical data and 103 illustrations including intensity-distribution curves, energy-level diagrams, and excellent reproductions of well chosen spectrograms. The book ends with a bibliography containing over 1700 classified references, and adequate indexes of authors and subjects. The page is larger than that used for previous monographs in this series—a decided improvement.

W. J.

*Light*, by F. Bray, M.A. 2nd ed. Pp. x + 369. (London: Edward Arnold and Co., 1938.) 7*s*. 6*d*.

The first edition of this book, which appeared in 1927, must have become so widely known to teachers and students that it is unnecessary to refer at length here to the scope and purpose of the new edition. In revising the work the author has adopted one of the two sign conventions recommended in the Physical Society's *Report on the Teaching of Geometrical Optics* (1934), in order that focal length may take the same sign as dioptric power. The chosen system is the non-Cartesian one in which real objects and images are regarded as being formed in positive spaces and virtual objects and images in negative spaces; and the author states that this "appeals to boys much more than one based on mathematical usage", i.e. either of the Cartesian systems, the old or the new. It is interesting to note that both the choice and the reason for it are the same as in another recent book of about the same scope, Noakes' *Text-book of Light*, which was reviewed earlier in the present volume of these *Proceedings* (p. 150). Additions have been made to every chapter and the author has had in mind the demand for more up-to-date instruction, mainly in illumination, diffraction and spectra. He has done rightly, in the reviewer's opinion, in placing the rewritten chapter on photometry much later in the book, at the end of Part I (geometrical optics).

It is surprising to find in any optical work, especially one in which a large number of short historical and biographical notes appears, that the author is so completely unaware of the whereabouts of the Rowland ruling engine that the distinguished research professor now using it in its much improved form appears (p. 284) as "Professor Wood of Alberta". In a future edition some of the diagrams, for instance figures 64, 118, 132, 139A, 146 and 228 (*a*), might be amended, and "travelled slower" (p. 10) be changed to "travelled more slowly". It is to be hoped that none of the copies supplied to the booksellers is as imperfect as the reviewer's copy, in which four pages (pp. v to viii, the preface and part of the table of contents) are duplicated, and two pages (pp. 353–4, giving the first few of the examples from Higher Certificate, Intermediate Science and Scholarship examination questions) are missing.

W. J.

*Das Mikroskop*, by A. Eringhaus. Pp. 156, 83 illustrations. (Berlin and Leipzig: B. G. Teubner.) Price in England RM. 2.70.

This little book is one of a series of mathematical-physical books published by B. G. Teubner of Leipzig and Berlin. Its aim is to give an account of the construction and use of the microscope and of its chief accessories. The underlying theory is clearly and concisely stated in an elementary manner but sufficiently to explain the relation between numerical aperture and resolving power, and Abbe's experiments on the effects of apertures placed in the upper focal plane of the objective on the resolution of gratings are described. After a very brief examination of the aberrations of the objective and eyepiece, methods of measuring their optical constants and the optical tube-length as well as of testing the definition and resolving power are given.

The description of the actual setting up of the instrument for ordinary observation seems unduly condensed, only some three and a half pages being devoted to it, so that it is doubtful if a beginner would find it sufficient. On the other hand a large number of special uses of the microscope are well, though again perhaps somewhat briefly, described: measurements, monochromatic and dark-ground illumination, the ultramicroscope, the use of ultra-violet light and the fluorescent microscope, as well as such matters as the camera lucida and projection. A further section describes the preparation of objects for observation—staining, section-cutting and mounting. Whilst therefore there is nothing new in this book, it should prove useful for the microscope-user, especially in the more modern and extended applications of the microscope, for which the literature is largely scattered in current periodicals.

R. S. C.

*Numerical Problems in Advanced Physical Chemistry*, by J. H. WOLFENDEN. Pp. xx+227. (Oxford: Clarendon Press, 1938.) 7*s*. 6*d*. net.

The subjects illustrated by the examples in the book are such as would be dealt with, in part, in advanced lecture courses in physical chemistry. The experiments concerned would be too difficult for the student to carry out, and unless research on them was in progress he would never see them. In such circumstances the value of numerical exercises in conjunction with lecture courses is considerable. Since the book does not give accounts of the underlying theory but gives a table of references to text books on physical chemistry, the author has been able to save much space and deal with more subjects than he otherwise could have done. He has also added useful notes to many of the problems. Many students find it very difficult to get any clear idea of practical applications from the text books and would have welcomed a brief restatement of the underlying principles before attacking the difficult problems in the book. Many of the subjects also are outside the scope of lecture courses which are possible in the limited time available, and some seem to be too highly specialized, as for example the section on crystal structure. The book will appeal most to the student beginning research in physical chemistry and the advanced student with a gift for physical chemistry and able to master the theories from the books referred to. It provides an excellent course in many parts of modern physical chemistry which are not covered in other books, and it will be welcomed by teachers and students. Its excellent printing and binding and low price are also deserving of praise.

J. R. P.

*Duodecimal Arithmetic*, by GEORGE S. TERRY. Pp. 29Ɛ. (London: Longmans, Green and Co., Ltd., 1938.) 30*s*. net.

This book consists primarily of tables of the commoner numerical functions, square roots, cube roots, reciprocals and so on, of the trigonometrical functions and of logarithms, all based on the duodecimal system in which counting is done by dozens instead of by tens. An introductory section explains the undoubted advantages of such a system, and it is to be regretted that the method is unlikely to come into general use owing to the complexities that would arise from the simultaneous existence of two systems. The author and publishers are to be congratulated on their enterprise in making such an excellently produced book available to those who wish to test the possibilities of duodecimal calculations. It may be noted that the pages of the book are numbered on the duodecimal system.

W. D. W.

*Time and its Importance in Modern Thought*, by M. F. CLEUGH, with a foreword by L. SUSAN STEBBING. Pp. viii + 308. (London: Methuen and Co.) 12*s*. 6*d*. net.

Time, as the writer herself points out, may be considered from many points of view. The physicist finds that events occur in time and finds the puzzle of time linked with that of motion. The psychologist finds that in man's mind is an awareness of the flow of time, and of the passage of events. In particular he finds a general belief that we move in time and meet events, which we then leave behind as in memory; few think of themselves as stationary, with the events passing by. Finally, there is the metaphysics of time. Dr Cleugh takes these aspects in order, with the metaphysics of Kant, Bergson, Alexander, McTaggart and Dunne each examined individually, and then attempts a synthesis of her own, culminating in a chapter on reality and its meaning.

Miss Cleugh is no more helpful than many older philosophers, yet many physicists will find the book interesting. They will find, however, that their interest is held, not as physicists, but because the book shows how philosophy regards some problems which are cognate to, but not identical with, some of their own.

J. H. A.

Under the general heading *Actualités Scientifiques et Industrielles* the Physical Society has received the monographs listed below. Each is written by an authority on his subject and the treatment is, in general, concise and clear. The publishers are Herman and Co., 6 Rue de la Sorbonne, 6, Paris.

516. PAUL RENAUD. *Analogies entre les Principes de Carnot, Mayer et Curie.* 10 fr.

517. M. HAÏSSINSKY. *Le Polonium.* 12 fr.

547. R. RIVAULT. *Contribution a l'Étude des Régions Ionisées de la Haute Atmosphère.* 20 fr.

549. LÉON BRILLOUIN. *La Structure des Corps Solides dans la Physique Moderne.* 18 fr.

550. LOUIS CARTAN. *Spectrographie de Masse. Les Isotopes et leurs Masses.* 20 fr.

621. HÉLÈNE METZGER. *Attraction Universelle et Religion Naturelle chez quelques Commentateurs Anglais de Newton.* 1ère Partie. *Introduction Philosophique.* 12 fr.

622. HÉLÈNE METZGER. *Attraction Universelle et Religion Naturelle chez quelques Commentateurs Anglais de Newton.* 2ème Partie. *Newton—Bentley—Whiston—Toland.* 15 fr.

623. HÉLÈNE METZGER. *Attraction Universelle et Religion Naturelle chez quelques Commentateurs Anglais de Newton.* 3ème Partie. *Clarke—Cheyne—Derham—Baxter—Priestley.* 25 fr.

# INDEX

PAGE

Acoustical conductivity of orifices, The 599
Aerodrome floodlight beacons, The flashing character of 642
Alfvén, Hannes: A simple scale-of-two counter 358
Alloys, An x-ray study of the superlattice in certain 389
Alpha, beta and gamma rays and neutrons, Demonstration of some effects produced by the irradiation of liquids and gels with 438
Aluminium, potassium chloride and copper for x rays, Atomic scattering factors of 96, 312
Asundi, R. K., M. Karim and R. Samuel: Emission bands of $SiCl_2$ and $SnCl_2$ 581
Atkinson, E. C.: Reply to discussion on p. 756 856
Atkinson, E. C.: The amplitude deviation of rate of a pendulum: a second experiment 742
Atkinson, E. C.: The dissipation of energy by a pendulum swinging in air 721
Atkinson, J. R., N. R. Campbell, E. H. Palmer and G. T. Wrinch: The accuracy of rectifier-photoelectric cells 934
Atomic scattering factors of aluminium, potassium chloride and copper for x rays 96, 312
Atomic vibrations in magnesium between 86° and 293° K., An x-ray investigation of 757
Attenuation and group retardation in the ionosphere 561
Auditory system, The nature of fatigue in the 108

Bailey, G. L. J., S. Fordham and J. T. Tyson: The surface structure of liquid mercury 63
Baker, C. J. W., *see* Bates, L. F.
Band system of SnO, Near ultra-violet 910
Bands of lead sulphide, A rotational analysis of 427
Bands of $SiCl_2$ and $SnCl_2$, Emission 581
Bate, A. E.: Note on the whispering gallery of St Paul's Cathedral, London 293
Bates, L. F. and C. J. W. Baker: The purification and magnetic properties of mercury 409
Beacons, The flashing character of aerodrome floodlight 642
Beats, A new demonstration of the phenomenon of 616
Beevers, C. A. and H. Lipson: On the evaluation of some hexagonal structure factors 275
Bell, H. and A. Harvey: A rotational analysis of bands of lead sulphide 427
Benjamin, M., R. J. Huck and R. O. Jenkins: Oxide-coated cathodes: I. Particle-size and thermionic emission 345
Bennett, G. A. and J. E. Calthrop: Demonstration of a laboratory method for the determination of the period of the transverse oscillations of flexible rods by means of a photoelectric cell 436
Best, J. E. and J. A. Ratcliffe: The diurnal variation of the ionospheric absorption of wireless waves 233
Beta and gamma rays and neutrons, Demonstration of some effects produced by the irradiation of liquids and gels with alpha, 438
$\beta$-ray spectra, The application of the absorption method to the determination of the upper limits of continuous 185
Betteridge, W.: The crystal structure of cadmium-indium alloys rich in indium 519
Bradford, S. C.: The kinetic theory of fluids 30
Brentano, J. C. M.: A comparison of the mixed-powder and substitution methods in the quantitative evaluation of x-ray reflections from crystalline powders 247, 312
Brindley, G. W. and P. Ridley: An x-ray investigation of atomic vibrations in magnesium between 86° and 293° K. 757
Brindley, G. W. and P. Ridley: An x-ray study of lattice-distortion in rhodium 501

PAGE

Brindley, G. W. and P. Ridley: Atomic scattering factors of aluminium, potassium chloride and copper for x rays . . . . . . . . . . . 96, 312
Brindley, G. W. and F. W. Spiers: The measurement in absolute units of the intensities of x-ray reflections from crystalline powders . . . . . . 17, 312

Cadmium fluoride, CdF, A note on the spectrum of . . . . . . . . 711
Cadmium-indium alloys rich in indium, The crystal structure of . . . . 519
Calthrop, J. E., *see* Bennett, G. A.
Campbell, Albert: The mutual shunt method of measuring self inductance at radio frequencies . . . . . . . . . . . . . . . . . 655
Campbell, N. R., *see* Atkinson, J. R.
Capillary tube, The motion of a mercury index in a . . . . . . . . 540
Capillary tubes, The propagation of supersonics in . . . . . . . . 553
Carbon, Thermionic emission from . . . . . . . . . . . 496
Cathodes, Diffraction of electrons by oxide-coated . . . . . . . . 635
Cathodes: 1. Particle-size and thermionic emission, Oxide-coated . . . . 345
Chalklin, F. C., S. S. Watts and S. P. Hillson: A concave-grating vacuum spectrograph for the wave-length 15 to 1000 A. . . . . . . . . . . 926
Chalmers, J. Alan and F. Pasquill: The electric charges of single raindrops and snowflakes . . . . . . . . . . . . . . . . . 1
Champion, F. C., *see* Widdowson, E. E.
Childs, C. B., *see* Farmer, F. T.
Chladni's original figures, Vibrations of free circular plates. Part 3: A study of . 83
CN molecule, Dissociation energy of the . . . . . . . . . . 283
Cobalt and iron as a function of temperature, Secondary-electron emission from nickel 625
Cockburn, R.: The variation of voltage-distribution and of electron transit time in the space-charge-limited planar diode . . . . . . . . . . 298
Cockburn, R.: The variation of voltage-distribution and of electron transit time in the space-charge-limited planar diode (Discussion) . . . . . . 476
Coin-tosses, A problem in . . . . . . . . . . . . . . 947
Colour sensations produced by ultra-violet light . . . . . . . . 714
Constable, J. E. R.: The prevention of the transmission of sound along water pipes 360
Constable, J. E. R.: The transmission of sound in a building by indirect paths . 368
Copper for x rays, Atomic scattering factors of aluminium, potassium chloride and 96, 312
Copper, The nuclear magnetic moment of . . . . . . . . . . 826
Cosmic-ray showers produced under thirty metres of clay, An investigation of . 783
Counter, A simple scale-of-two . . . . . . . . . . . . 358
Cowie, A., *see* Farmer, F. T.
Crawshaw, J. D.: An investigation of cosmic-ray showers produced under thirty metres of clay . . . . . . . . . . . . . . . 783
Cryostat, A simple type of helium . . . . . . . . . . . 525
Crystal reflections, The integration of large numbers of x-ray . . . . . 919
Crystal structure of cadmium-indium alloys rich in indium, The . . . . 519
Crystalline powders, A comparison of the mixed-powder and substitution methods in the quantitative evaluation of x-ray reflections from . . . . . 247
Crystalline powders, The measurements in absolute units of the intensities of x-ray reflections from . . . . . . . . . . . . . 17, 312

Darbyshire, James A.: Diffraction of electrons by oxide-coated cathodes . . 635
Daunt, J. G. and K. Mendelssohn: A simple type of helium cryostat . . . 525
Dawton, R. H. V. M.: The elastic properties of sodium wires between − 183° and 90° C. . . . . . . . . . . . . . . . . . 483
Dawton, R. H. V. M.: The integration of large numbers of x-ray crystal reflections 919
Demonstration of a laboratory method for the determination of the period of the transverse oscillations of flexible rods by means of a photoelectric cell . . 436

PAGE

Demonstration of a plane-electrode demountable thermionic tube used as a generator of ultra-high-frequency oscillations . . . . . . . 146
Demonstration of magneto-damping in nickel . . . . . . . . 144
Demonstration of structures proposed for protein molecules . . . . 141
Demonstration of the phenomenon of beats, A new . . . . . . . 616
Dielectrics, The construction of molecular models of . . . . . . . 852
Diffraction of electrons by oxide-coated cathodes . . . . . . . 635
Diode (Discussion), The variation of voltage-distribution and of electron transit time in the space-charge-limited . . . . . . . . . . 476
Diode, The variation of voltage-distribution and electron transit time in the space-charge-limited . . . . . . . . . . . . . 298
Dissociation energy of the CN molecule . . . . . . . . . 283

Edmunds, I. G., *see* Owen, Professor E. A.
Elastic properties of sodium wires between − 183° and 90° C., The . . . 483
Electric charges of single raindrops and snowflakes, The . . . . . . 1
Electron and the derivation of the quantum equation, The analogy between the photon and the . . . . . . . . . . . . . 899
Electron-diffraction and surface structure, Discussion on . . . . . 961
Electron-diffraction by the split-shutter method and a new (back-reflection) method . . . . . . . . . . . . . . . 776
Electron emission from nickel, cobalt and iron as a function of temperature, Secondary- . . . . . . . . . . . . . . . 625
Electron transit time in the space-charge-limited planar diode (Discussion), The variation of voltage-distribution and of . . . . . . . . 476
Electron transit time in the space-charge-limited planar diode, The variation of voltage-distribution and of . . . . . . . . . . . 298
Electrons by oxide-coated cathodes, Diffraction of . . . . . . . 635
Electrostatic field between two cylinders, The focusing properties of the . . 888
Ellis, C. D.: A note on nuclear selection rules . . . . . . . . 213
Equilibrium of a ball supported by a vertical air-jet, The . . . . . 385
Essen, L.: A new form of frequency and time standard . . . . . . 413

Farmer, F. T., C. B. Childs and A. Cowie: Critical-frequency measurements of wireless waves reflected obliquely from the ionosphere . . . . . 767
Flint, H. T.: The analogy between the photon and the electron and the derivation of the quantum equation . . . . . . . . . . . 899
Flint, H. T.: The ratio of the masses of the fundamental particles . . . 90
Flint, H. T. and W. Wilson: The fundamental unit of electric charge . . 340
Fluids, The kinetic theory of . . . . . . . . . . . . 30
Fordham, S., *see* Bailey, G. L. J.
Forester, G. O., *see* Tolansky, S.
Frequency and time standard, A new form of . . . . . . . . . 413
Frequency measurements of wireless waves reflected obliquely from the ionosphere, Critical- . . . . . . . . . . . . . 767
Frequency of vibration of molecules in liquids and its relation to viscosity, The . 788

Galloway, W. R., *see* Jones, F. Llewellyn
$\gamma$-radiation produced by slow neutrons, Intensity of . . . . . . . 534
Gamma rays and neutrons, Demonstration of some effects produced by the irradiation of liquids and gels with alpha, beta and . . . . . . 438
Gaydon, A. G.: Colour sensations produced by ultra-violet light . . . 714
Gaydon, A. G., *see also* Pearse, R. W. B.
Gerö, L., *see* Schmid, R.
Group retardation in the ionosphere, Attenuation and . . . . . . 561

PAGE

Hamilton, R. A., *see* Whatman, A. B.
Hampton, W. M. and J. G. Holmes: The flashing character of aerodrome floodlight beacons . . . . . . . . . . . . . 642
Harrison, E. P. and H. Rowe: An impedance magnetometer . . . . . 176
Hartshorn, L.: The construction of molecular models of dielectrics . . . 852
Harvey, A., *see* Bell, H.
Heat-conduction in a medium having thermal properties depending on the temperature . . . . . . . . . . . . . . . 703
Heat of nickel from 100° C. to 600° C., The specific . . . . . . . 834
Helium cryostat, A simple type of . . . . . . . . . . . 525
Hill, R. D. and A. A. Townsend: Intensity of $\gamma$-radiation produced by slow neutrons . . . . . . . . . . . . . . . 534
Hillson, S. P., *see* Chalklin, F. C.
Holmes, J. G., *see* Hampton, W. M.
Hopkins, M. R.: Heat-conduction in a medium having thermal properties depending on the temperature . . . . . . . . . . . 703
Hopwood, F. L. and J. T. Phillips: Demonstration of some effects produced by the irradiation of liquids and gels with alpha, beta and gamma rays and neutrons . . . . . . . . . . . . . . . 438
Huck, R. J., *see* Benjamin, M.
Hunter, A. and R. W. B. Pearse: Sensitivity of photographic plates in the region 2500–2000 A. . . . . . . . . . . . . . . 256

Indium, The crystal structure of cadmium-indium alloys rich in . . . . 519
Ionosphere, Attenuation and group retardation in the . . . . . . 561
Ionosphere, Critical-frequency measurements of wireless waves reflected from the 767
Ionospheric absorption of wireless waves, The diurnal variation of the . . . 233
Ionospheric transmission phenomena at oblique incidence and those at vertical incidence. The relation between . . . . . . . . . . . 801
Iron as a function of temperature, Secondary-electron emission from nickel, cobalt and . . . . . . . . . . . . . . . 625
Irradiation, Monochromators for purposes of . . . . . . . . 265
Irradiation of liquids and gels with alpha, beta and gamma rays and neutrons, Demonstration of some effects produced by the . . . . . . . 438

Jackson, L. C.: The principal paramagnetic susceptibilities of potassium ferricyanide at low temperatures . . . . . . . . . . . . 707
Jackson, R. and A. G. Quarrell: Electron-diffraction by the split-shutter method and a new (back-reflection) method . . . . . . . . . 776
Jackson, Sir Herbert, Obituary notice of . . . . . . . . . 467
Jenkins, R. O., *see* Benjamin, M.
Jet, The equilibrium of a ball supported by a vertical air- . . . . . . 385
Jevons, W.: A note on the near ultra-violet band system of SnO . . . . 910
Jones, F. Llewellyn and W. R. Galloway: The sparking potential of mercury vapour . . . . . . . . . . . . . . . . 207

Karim, M., *see* Asundi, R. K.

Landon, D. H., *see* Treloar, L. R. G.
Lattice-distortion in rhodium, An X-ray study of . . . . . . . 501
Lead sulphide, A rotational analysis of bands of. . . . . . . . . 427
Leyshon, W. A.: Demonstration of a plane-electrode demountable thermionic tube used as a generator of ultra-high-frequency oscillations . . . 146
Lighting, The appraisement of . . . . . . . . . . . . 119
Lipson, H., *see* Beevers, C. A.

PAGE

Luminosity of radiation at wave-lengths 5780 and 5461 A. for the average photometric observer, The relative . . . . . . . . . . 398
Lythgoe, R. J.: The structure of the retina and the role of its visual purple . 321

Macleod, D. B.: The frequency of vibration of molecules in liquids and its relation to viscosity . . . . . . . . . . . . . 788
Magnesium between 86° and 293° K., An X-ray investigation of atomic vibrations in 757
Magnetic moment of copper, The nuclear . . . . . . . . . 826
Magnetic properties of mercury, The purification and . . . . . . 409
Magneto-damping in nickel, Demonstration of . . . . . . . . 144
Magnetometer, An impedance . . . . . . . . . . . . 176
Manganese hydride, MnH, The spectrum of . . . . . . . . 201
May, J.: The propagation of supersonics in capillary tubes . . . . 553
Mendelssohn, K., *see* Daunt, J. G.
Mercury index in a capillary tube, The motion of a . . . . . . . 540
Mercury, The purification and magnetic properties of . . . . . . 409
Mercury, The surface structure of liquid . . . . . . . . . 63
Mercury vapour, The sparking potential of . . . . . . . . . 207
Metals: Part 1, tin (Discussion), The rate of viscous flow of . . . . . 311
Miller, H. and J. W. Strange: The electrical reproduction of images by the photoconductive effect . . . . . . . . . . . . 374
Millington, G.: Attenuation and group retardation in the ionosphere . . . 561
Millington, G.: The relation between ionospheric transmission phenomena at oblique incidence and those at vertical incidence . . . . . . . 801
Monochromators for purposes of irradiation . . . . . . . . . 265
Mott, N. F.: Note on the theory of photoconductivity . . . . . . 196

Nelson, J. H.: Anomalous trichromatism and its relation to normal trichromatism 661
Neutrons, Demonstration of some effects produced by the irradiation of liquids and gels with alpha, beta and gamma rays and . . . . . . . 438
Neutrons, Intensity of γ-radiation produced by slow . . . . . . . 534
Nickel, cobalt and iron as a function of temperature, Secondary-electron emission from . . . . . . . . . . . . . . . . 625
Nickel, Demonstration of magneto-damping in . . . . . . . . 144
Nickel from 100° C. to 600° C., The specific heat of . . . . . . . 834
Nicoll, F. H.: The focusing properties of the electrostatic field between two cylinders . . . . . . . . . . . . . . . 888
Nuclear selection rules, A note on . . . . . . . . . . 213

Obituary notices . . . . . . . . . . . . 441, 467, 968
Optical instruments, Vision through . . . . . . . . . . 863
Orifices, The acoustical conductivity of . . . . . . . . . 599
Oscillations of flexible rods by means of a photoelectric cell, Demonstration of a laboratory method for the determination of the period of the transverse . 436
Owen, Professor E. A. and I. G. Edmunds: An X-ray study of the superlattice in certain alloys . . . . . . . . . . . . . 389

Palmer, E. H., *see* Atkinson, J. R.
Paramagnetic susceptibilities of potassium ferricyanide at low temperatures, The principal . . . . . . . . . . . . . . 707
Parker, R. C.: The nature of fatigue in the auditory system . . . . 108
Particles, The ratio of the masses of the fundamental . . . . . . 90
Pasquill, F., *see* Chalmers, J. Alan
Paterson, C. C.: The appraisement of lighting . . . . . . . . 119
Pearse, R. W. B. and A. G. Gaydon: A note on the spectrum of cadmium fluoride, CdF 711

PAGE

Pearse, R. W. B. and A. G. Gaydon: The spectrum of manganese hydride, MnH . . . . . . . . . . . . . 201
Pearse, R. W. B., *see also* Hunter, A.
Pendulum: a second experiment, The amplitude deviation of rate of a . . . 742
Pendulum swinging in air, The dissipation of energy by . . . . . . . 721
Perry, J. W.: Monochromators for purposes of irradiation . . . . . . 265
Phillips, J. T., *see* Hopwood, F. L.
Photoconductive effect, The electrical reproduction of images by the . . . 374
Photoconductivity, Note on the theory of . . . . . . . . . . 196
Photoelectric cell, Demonstration of a laboratory method for the determination of the period of the transverse oscillations of flexible rods by means of a . 436
Photoelectric cells, The accuracy of rectifier- . . . . . . . . 934
Photographic plates in the region 2500–2000 A., Sensitivity of . . . . . 256
Photometric observer, The relative luminosity of radiation at wave-lengths 5780 and 5461 A. for the average . . . . . . . . . . . 398
Photon and the electron and the derivation of the quantum equation, The analogy between . . . . . . . . . . . . . . . . 899
Potassium chloride and copper for X rays, Atomic scattering factors of aluminium 96, 312
Potassium ferricyanide at low temperatures, The principal paramagnetic susceptibilities of . . . . . . . . . . . . . . . 707
Presidential Address: Vision through optical instruments . . . . . . 863
Preston, J. S.: The relative luminosity of radiation at wave-lengths 5780 and 5461 A. for the average photometric observer . . . . . . . 398
Probability, Discussion on . . . . . . . . . . . . . 954
Probability paper, Note on the use of . . . . . . . . . . 914
Protein molecules, Demonstration of structures proposed for . . . . . 141

Quantum equation, The analogy between the photon and the electron and the derivation of the . . . . . . . . . . . . . 899
Quarrell, A. G., *see* Jackson, R.

Radioactive source, A note on the optimum counting-time for measuring the intensity of a . . . . . . . . . . . . . . 87
Radio observations, High latitude . . . . . . . . . . . 217
Raindrops and snowflakes, The electric charges of single . . . . . . 1
Ratcliffe, J. A., *see* Best, J. E.
Rectifier-photoelectric cells, The accuracy of . . . . . . . . . 934
Reimann, A. L.: Thermionic emission from carbon . . . . . . . 496
Reproduction of images by the photoconductive effect, The electrical . . 374
Retina and the role of its visual purple, The structure of the . . . . . 321
Reviews of books . . . . . . . . . . . 149, 313, 478, 618, 858, 976
Rhodium, An X-ray study of lattice-distortion in . . . . . . . . 501
Richards, D. A.: A new demonstration of the phenomenon of beats . . . 616
Ridley, P., *see* Brindley, G. W.
Robinson, N. W.: The acoustical conductivity of orifices . . . . . . 599
Rods by means of a photoelectric cell, Demonstration of a laboratory method for the determination of the period of the transverse oscillations of flexible . 436
Rowe, H., *see* Harrison, E. P.
Rutherford of Nelson, New Zealand and Cambridge, Ernest, first Baron, Obituary notice of . . . . . . . . . . . . . . . 441

St Paul's Cathedral, London, Note on the whispering gallery of . . . . 293
Samuel, R., *see* Asundi, R. K.
Savur, S. R.: A problem in coin-tosses . . . . . . . . . . 947
Schmid, R., L. Gerö and J. Zemplén: Dissociation energy of the CN molecule . 283

PAGE

Self inductance at radio frequencies, The mutual shunt method of measuring 655
Selwyn, E. W. H.: Note on the use of probability paper 914
$SiCl_2$ and $SnCl_2$, Emission bands of 581
Smith, Thomas: Vision through optical instruments (Presidential Address) 863
$SnCl_2$, Emission bands of $SiCl_2$ and 581
SnO, A note on the near ultra-violet band system of 910
Snowflakes, The electric charges of single raindrops and 1
Sodium wires between −183° and 90° C., The elastic properties of 483
Sound along water pipes, The prevention of the transmission of 360
Sound in a building by indirect paths, The prevention of the transmission of 368
Sparking potential of mercury vapour, The 207
Spectrograph for the wave-length 15 to 1000 A., A concave-grating vacuum 926
Spectrum of cadmium fluoride, CdF, A note on the 711
Spectrum of manganese hydride, MnH, The 201
Spiers, F. W., *see* Brindley, G. W.
Strange, J. W., *see* Miller, H.
Structure factors, On the evaluation of some hexagonal 275
Superlattice in certain alloys, An X-ray study of the 389
Supersonics in capillary tubes, The propagation of 553
Surface structure, Discussion on electron-diffraction and 961
Surface structure of liquid mercury, The 63
Sykes, C. and H. Wilkinson: The specific heat of nickel from 100° C. to 600° C. 834

Tandberg, J.: A note on the optimum counting-time for measuring the intensity of a radioactive source 87
Thermionic emission from carbon 496
Thermionic emission, Oxide-coated cathodes: 1. Particle-size and 345
Thermionic tube used as a generator of ultra-high-frequency oscillations, Demonstration of a plane-electrode demountable 146
Time standard, A new form of frequency and 413
Tin (Discussion), The rate of viscous flow of metals: Part 1 311
Tin, The rate of viscous flow of metals: Part 1 153
Tolansky, S. and G. O. Forester: The nuclear magnetic moment of copper 826
Townsend, A. A., *see* Hill, R. D.
Treloar, L. R. G. and D. H. Landon: Secondary-electron emission from nickel, cobalt and iron as a function of temperature 625
Trichromatism and its relation to normal trichromatism, Anomalous 661
Tyson, J. T., *see* Bailey, G. L. J.
Tyte, L. C.: The rate of viscous flow of metals: Part 1, tin 153
Tyte, L. C.: The rate of viscous flow of metals: Part 1, tin (Discussion) 311

Ultra-violet light, Colour sensations produced by 714
Unit of electric charge, The fundamental 340

Vibrations of free circular plates. Part 1: Normal modes 70
Vibrations of free circular plates. Part 2: Compounded normal modes 77
Vibrations of free circular plates. Part 3: A study of Chladni's original figures 83
Viscosity, The frequency of vibration of molecules in liquids and its relation to 788
Viscous flow of metals: Part 1, tin (Discussion), The rate of 311
Viscous flow of metals: Part 1, tin, The rate of 153
Vision through optical instruments 863
Voltage-distribution and of electron transit time in the space-charge-limited planar diode (Discussion), The variation of 476
Voltage-distribution and of electron transit time in the space-charge-limited planar diode, The variation of 298

PAGE
Waller, Mary D.: Demonstration of magneto-damping in nickel . . . . 144
Waller, Mary D.: Vibrations of free circular plates. Part 1: Normal modes . 70
Waller, Mary D.: Vibrations of free circular plates. Part 2: Compounded normal modes . . . . . . . . . . . . . . 77
Waller, Mary D.: Vibrations of free circular plates. Part 3: A study of Chladni's original figures . . . . . . . . . . . . . 83
Watts, S. C., *see* Chalklin, F. C.
Whatman, A. B. and R. A. Hamilton: High latitude radio observations . . 217
Widdowson, E. E. and F. C. Champion: The application of the absorption method to the determination of the upper limits of continuous $\beta$-ray spectra . . 185
Wilkinson, H., *see* Sykes, C.
Winch, G. T., *see* Atkinson, J. R.
Wireless waves reflected from the ionosphere, Critical-frequency measurements of 767
Wireless waves, The diurnal variation of the ionospheric absorption of . . 233
Wrinch, D. M.: Demonstration of structures proposed for protein molecules . 141

X-ray crystal reflections. The integration of large numbers of . . . . 919
X-ray investigation of atomic vibrations in magnesium between 86° and 293° K., An 757
X-ray reflections from crystalline powders, A comparison of the mixed-powder and substitution methods in the quantitative evaluation of . . . . . . 247
X-ray reflections from crystalline powders, The measurement in absolute units of the intensities of . . . . . . . . . . . . . 17, 312
X-ray study of lattice-distortion in rhodium . . . . . . . . 501
X-ray study of the superlattice in certain alloys, An . . . . . . 389
X rays, Atomic scattering factors of aluminium, potassium chloride and copper for 96

Yarnold, G. D.: The equilibrium of a ball supported by a vertical air-jet . . 385
Yarnold, G. D.: The motion of a mercury index in a capillary tube . . . 540

Zemplén, J., *see* Schmid, R.

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